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Antiquity

A QUARTERLY REVIEW OF ARCHAEOLOGY



Edited by Glyn Daniel

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Antiquity

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Editorial

TWO very controversial archaeological issues have been much discussed in the British Press in the last few months. The first is the suggestion—not made now for the first time—that the Elgin Marbles should be returned to Greece; the second that Stonehenge should be prevented from becoming a centre of neo-Druidism and hooliganism at Midsummer. The controversy about the return of the Elgin Marbles was started on this occasion by Mr Francis Noel Baker, M.P., who asked the Prime Minister if the Government would return the marbles to Greece. Mr Macmillan's refusal was backed up by a leader in *The Times*. A heated discussion followed. On one side it was said that as we had rescued the marbles we had a right to keep them, and that they are better preserved in a museum than being left on the Acropolis at Athens. (Those left on the Parthenon have badly weathered, true, but does it have to be a museum in London?) On the other side it was argued that the marbles were an integral part of the Parthenon and that our title to them was uncertain since they were removed with Turkish, not Greek, permission.

The facts about the removal of the marbles from Athens to Bloomsbury are not in dispute. The Earl of Elgin was Ambassador to Constantinople from 1799 to 1803. He very wisely used his mission for archaeological research and from 1800 onwards had a band of artists drawing, measuring, recording, and taking casts of the antiquities of Greece. In his *Greek Studies in England, 1700–1830*, M. L. Clarke writes, 'At first Elgin's intention was only to make drawings and casts, not to take away any of the sculpture, but as he learnt more of conditions in Athens and saw the constant destruction and defacement to which the antiquities were subject, he changed his plan, and determined to remove as much of the sculpture as he could.' In 1801 the Porte gave him a firman which included permission to take away 'any pieces of stone with inscriptions or figures'. The Elgin collection included, in addition to the great majority of the sculptures from the Parthenon, four reliefs from the temple of Nike, and one of the Caryatids from the Erechtheum. It might have contained much more. His chaplain, Dr Hunt, when supervising the work of clearing the Caryatid porch of the Erechtheum, wrote to Elgin, 'If your Lordship would come here in a large Man of War, that beautiful little model of ancient art might be transported wholly to England'. Elgin's collection was shipped to England, and in 1807 was displayed in a room at the back of his house in Park Lane; it was bought for the British Museum for £35,000 in 1816.

Elgin himself was quite sure of what he was doing and why; it was to rescue ancient Greek art for posterity. 'The Turks have been continually defacing the heads', he wrote,

'and in some instances they have actually acknowledged to me that they have pounded down the statues to convert them into mortar. It was upon these suggestions and with these feelings that I proposed to remove as much of the sculpture as I conveniently could.' It was clear to him at the beginning of the 19th century, as it appeared clear to many for decades after this, that it was our duty as a nation to collect and house art treasures from other countries too backward or too negligent to look after them themselves. The famous second leader of *The Times* for 4 November, 1861, which begins with the passage 'The crowded state of the British Museum has been for a long time the reproach not so much of any particular person or set of persons as of the administrative system under which these vast national treasures are placed . . .' contains a splendid passage about the problems of housing in London the treasures of the world. 'We have got to lodge Halicarnassus, Cyrene and Carthage and we absolutely know not where to put them. The impending disruption, if not the present feebleness of the Turkish Empire, seems about to throw open to us all the many treasures of Asia Minor. Who knows what objects of art may be brought to light by the exertions of a liberal and energetic Government in the whole territory of Naples, where every step the traveller takes is over crumbling ruins of ancient Greek and Roman art? Who can tell what may be found in the bed of the Tiber, from the candlesticks of the Temple of Jerusalem to the masterpieces of PHIDIAS and PRAXITELES? We are going to Mexico: what may we not discover in the halls of the MONTEZUMAS or in the buried treasures of Palenque! Egypt is but half-ransacked, and Arabia and Mesopotamia have still much with which to reward the exertions of an intelligent traveller.'

This was 1861—fifty years after E. Daniel Clarke had exclaimed on finding what he thought was the 'Tomb of Euclid' in Athens, 'Such an antiquity must be for the University of Cambridge, where the name of Euclid is so particularly revered', and the very time when the Mudir of Keneh was stealing Mariette's finds, Belzoni and Drouetti were fighting each other for possession of an obelisk and Rassam was pirating other people's excavations to find Assyrian treasures for the British Museum. We would not do these things now; we now possess and parade an archaeological conscience, the development of which has been studied so interestingly by Seton Lloyd in the last two chapters of his *Foundations in the Dust*.

The real issue is this: does our archaeological conscience work retrospectively to change what we did in the past when viewed in the circumstances of the present? In his *Journey through Albania*, written in 1810, Hobhouse said: 'I have said nothing of the possibility of the ruins of Athens being, in the event of a revolution in the favour of the Greeks, restored and put into a condition capable of resisting the ravages of decay; for an event of that nature cannot, it strikes me, have even entered the head of anyone who has seen Athens and the modern Athenians.' By today Greece has been an independent country for 140 years and a great number of archaeologists have seen 'Athens and the modern Athenians'. It does enter the heads of all of us that the Elgin Marbles might now in justice go back to Athens where they belong and where there is a modern, vigorous nation deeply conscious of its ancient past and aware of the need for conserving and protecting its antiquities.

But if we begin with the Elgin Marbles, where do we stop? The Rosetta Stone must surely by the same token go back to Egypt. Indeed, it has less claim to be in the British Museum than have the Elgin Marbles. Found by accident in 1799 by a French soldier digging near the old Fort Rashid five miles from Rosetta in the Nile Delta, it was taken to Cairo and studied by the scholars of Napoleon's Institut d'Egypte. Later it was placed in the private house of the French General, Jacques-François de Menou. General Menou

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was forced to capitulate to the British Army in 1801 and the French were ordered to give up all the Egyptian antiquities acquired by them during the previous three years. Attempts were made to keep the Rosetta Stone by saying it was the private property of General Menou and outside the terms of the surrender. But the English General, Lord Hutchinson, insisted on the stone being handed over; it arrived in Portsmouth the following year and was sent to the British Museum, where, wrote Turner in his report on it, 'we hope it will remain for many years . . . as a glorious trophy of British arms . . . not captured from a defenceless population, but conquered honourably and according to the rules of warfare'. *Many years? How many years?* It is the rules of archaeological warfare that need formulating, and the ethics of the retention of other peoples' archaeological treasures that need thinking out. But with the full knowledge that a logical application of these rules and principles might mean that it is the Louvre and the British Museum which would be half-ransacked!



Stonehenge is our own national affair, and what we do with it and to it is perhaps no direct concern of any other nation. The Greeks are not going to say that, because it may be inspired by Mycenaean architecture and may have on it the representation of a Mycenaean dagger, Stonehenge should be set up in the Peloponnese. Even the most rabid Welsh nationalist born on the slopes of the Presely mountains is not going to say, 'Give us back our stones'. But the British Government, through its Minister of Works, guards Stonehenge in trust for all posterity. In their Annual Report for 1960 the Ancient Monuments Board for England suggest very clearly to the Minister of Works that all is not well at Stonehenge during the annual Midsummer celebrations. They say to him, 'While we see no objection to you allowing a ceremony to be held, we deprecate the behaviour of some of the large crowd of onlookers and fear the possible harm to the monument. Last summer spectators forced their way through the boundary fence and a large crowd assembled in the centre of the monument. Many individuals climbed the stones to use them as vantage points and outside the boundary visitors parked their cars on prehistoric earthworks. As much of this activity takes place in darkness, we fear that there is a serious risk of accidents to a supremely important monument. There is no excuse for anyone at any time forcing their way through the fences and there should be an absolute prohibition on climbing the stones.'

This report was published just before Midsummer this year and we waited with considerable alarm to learn what happened at Stonehenge at the summer solstice of 1961. We quote from the report in *The Times*: 'Nearly 3000 people, it is estimated, watched the dawn ceremony on this day of longest light. As on previous occasions, unruly elements sought to lessen the dignity of the proceedings by their behaviour. On the lighter side was the man who on being asked by a custodian for his 6d. entry ticket replied that he had not bought one as he was only attending a party in the circle to which he had been invited. More serious was the litter of broken bottles—eight barrowfuls were later removed from the enclosure—and the groups of jeering louts perched on top of the stones. Strenuous efforts by the police kept the dawn ceremony moving, but lack of numbers prevented them from keeping more than a watching brief on earlier antics, which began in the late hours yesterday and continued until after the dawn broke. A strong force of military police was also on duty.'

This is a monstrous, wicked and most undesirable state of affairs and one which the Minister must bring to an end; and certainly by next Midsummer. We see *every* objection to his allowing a ceremony to be held and for two good reasons. The first is that the

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Ancient Order of Druids has no claim to an association with Stonehenge, and the second that the existence of these strange ceremonies attracts crowds some of whom behave deplorably. We are no spoil-sport and we love the minor faiths that flourish away from the main stream of religion as we love the lunacies that flourish away from the main stream of archaeology; but Dr R. MacGregor-Reid, Chief Druid of the Companions of the Ancient Order of Druids, should be told that, in the best interests of Stonehenge and British Archaeology, the Minister of Works can no longer give permission for these grave Midsummer antics. Next June there should be, we suggest, no admission to the monument at Midsummer and a completely adequate guard of civil and military police. After all, Dr MacGregor-Reid and his Druids can do what is done by the Gorsedd of Bards of the Welsh National Eisteddfod, who build their own stone circle in a different place each year.

The Ministry of Works, we understand, is asked each year by the body concerned for permission to hold a ceremony at Stonehenge. The line that has been taken by the department ever since Stonehenge came into its care in 1919 is that any applicants may hold a religious ceremony at the monument provided they pay the appropriate entrance fee and observe the department's general rules. It is understood that in the past forty years quite a number of bodies claiming the name of Druid have sought and received permission on this basis. All these claimants, we feel, should no longer receive this permission, on grounds of archaeology and the conservation of our ancient monuments. We do not expect the Minister and his advisors and staff to evaluate what of religion and what of fantasy, what of truth and what of rubbish, exists in the claims of these bodies. They are all foolish people confusing fact with fiction. If it makes them happy—splendid. But their private happiness must not endanger one of our great prehistoric monuments—a piece of our ancient heritage which it is our duty, from the Minister of Works downwards, to conserve and preserve.



The protection and preservation of the material heritage of the past presents, of course, to every nation a great problem. Stonehenge on Midsummer night is perhaps a small and domestic issue. The objects and sites of antiquity threatened by the raising of the River Nile as a result of the completion of the High Dam at Aswan in 1970 are, like the Parthenon sculptures now in Bloomsbury and the Rosetta Stone, matters in which we are all concerned. Of these sites the two temples at Abu Simbel are the most impressive and pose the most complicated technical problem. Everyone will have read with great interest the details of the French-sponsored scheme to surround the temples with a dam curving out from the present line of the river, and the Italian project to cut both temples away from the rock, enclose them temporarily in huge concrete boxes and elevate them 190 ft. so that they would then stand above the new level of the new Nile.

Surely the Italian project is the thing to do and it is good to know that on 20 June the Government of the United Arab Republic accepted the advice of international engineers to adopt this plan, described by Sir Mortimer Wheeler in a recent article in *The Times* (7 July, 1961), as 'this unparalleled and indeed almost frightening feat of engineering'. The cost of this operation is likely to be £20 millions or 60 to 70 million dollars. An angry letter in *The Times* for 26 June, 1961, declares that if 'Egypt thinks it necessary to attract the tourist by giving him something *kolossal und wunderbar* at Abu Simbel . . . the cost of it should surely be the affair of none except the country in which Abu Simbel is situated'. Professor Mallowan, in his wise reply (*The Times*, 28 June, 1961), says that

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Abu Simbel 'a unique, incomparable monument . . . which for over 3000 years has enshrined the spirit of ancient Egypt, is the heritage not of one country but of all mankind'. And in the article we have just quoted from, Sir Mortimer says the same thing with eloquence and force: 'There is nothing in 3000 years of Egyptian civilization quite comparable with this. It is the veritable entry into the underworld to which Pharaonic Egypt gave so much of its thinking. . . . To lose it would be irreparable loss. . . . Failure would mean something more than the loss of Abu Simbel: it would be in a real sense a defeat of the human spirit'.

We must see that this does not happen. The Egyptian Government is giving 10 million dollars towards the cost of this unparalleled and frightening feat of engineering, and no doubt other governments will give comparable sums. (We can lobby to see that they do.) But there will be a large sum remaining. This must be produced by private subscription from all over the world. The hot-dog seller outside the Ashmolean said to us recently: 'Wot's all this about raising an antique in Egypt? Is it O.K. by you? Yaas? Fine. Then have this hot-dog on me and put the money in the fund.' This is, of course, the spirit in which we should all approach these problems, from the Elgin Marbles and the Rosetta Stone to Stonehenge and Abu Simbel. They are all part of our common heritage of the past, in whatsoever nation-state boundaries they may now be confined. No country is an archaeological island, and hardly any modern states can pay for all the major archaeological repairs and restorations they want to do or ought to want to do. But Abu Simbel is exceptional; because it is being destroyed and because sentimentalists, opportunists and politicians will say that this great sum of money should be used instead for improving the standard of living of the *fellahin*. But this is to confuse the economy of one state in the 20th century with the heritage of all centuries and to muddle our debt to our fellow men with our debt to our past. We may never be able to deal properly with our fellow men, but we need not wilfully deny the possibility of discharging our duties to our ancestors. Abu Simbel is different in kind and urgency from the Elgin Marbles, the Rosetta Stone and Stonehenge, though they all are aspects of the central question—how *do* we deal with the heritage of the past? The overall contract for the elevation of this wonderful monument must be given by 1 January, 1962, and UNESCO must be able to give the go ahead by the end of September, 1961. As we write there is no sign of a world-wide appeal for funds, but perhaps by the time we are in print such a fund will have been launched. Five million archaeologists and their friends who would forego their hot-dogs for a while (and much else), could produce a very large sum of money and also make a gesture which showed that we all realize the common ancient heritage of all civilization, and are not prepared to allow that defeat of the human spirit which the submergence of Abu Simbel would mean.

Sir John Marshall and Gandhāra Art

by DANIEL SCHLUMBERGER

In ANTIQUITY for 1959 we published an account of his excavations at Surkh Kotal in Afghanistan by Professor Daniel Schlumberger, Director of the French Archaeological Mission in Afghanistan and Professor in the University of Strasbourg. In this article Professor Schlumberger discusses Sir John Marshall's views of Gandhāra art as set out in his The Buddhist art of Gandhāra, the story of the Early School, its birth, growth and decline (Memoirs of the Department of Archaeology in Pakistan, Volume I: Cambridge, the University Press: pp. xviii + 112, 152 photographs. 1960, 45s.).

AS the Director of the Archaeological Survey of India for more than thirty years, at a time when new finds of Gandhāra art were constantly pouring in, as the excavator of Taxila, as an excavator in Gandhāra itself, Sir John Marshall has had a unique opportunity of building up first-hand knowledge of Gandhāra art. That his views on this great subject should be made known was most desirable, and the Department of Archaeology in Pakistan is to be thanked for having undertaken to publish this volume.

The illustration is abundant and, nearly all of it, excellent. It consists of 152 photographs, of sufficient size (in happy contrast with most of the pictures in Foucher's *Art gréco-bouddhique*, and in some of Sir John Marshall's own publications, *Taxila* included), carefully selected and arranged in order to exemplify the development of 'the Early School', as seen by Sir John. Many a piece is here properly reproduced for the first time. As a compendium of good pictures the volume will be most useful.

But the book is far more than just an album of well-chosen photographs, with commentary. As the subtitle says, it is a *story*. This story is told in twelve chapters. An introduction (Chapter 1) deals with the beginnings of Buddhism in India, and with the problem of *how Buddhism fared under the Greek princes of the North-West during the second century B.C.* . . . *Among the myriads of Buddhist monuments that are preserved there is not one that can be referred with certainty to Greek authorship* in that period (p. 4). Chapter 2 is about *the Early Indian school of Art*. Then come three chapters on the origins: Chapter 3, *The Beginning of Gandhāra art: the Śāka period*; Chapter 4, *The Renaissance of Hellenistic art under the Parthians and its effect on Gandhāra art*; Chapter 5, *Childhood of Gandhāra art*. To *Adolescence* two chapters are devoted (Chapters 6 and 7), to *Maturity* four: Chapter 8 deals with the *Period of Maturity* in general; Chapter 9 with the *Early Maturity period*; the two following ones deal with the *Later Maturity period*, Chapter 10 with the *reliefs*, and Chapter 11 is about *images and decorative carvings*. Chapter 12 gives the conclusions.

The first point to be discussed here is Sir John Marshall's conception of an *Early School* using *almost exclusively . . . stone suitable for fine carving*, as contrasted with a *Later School* or *Indo-Afghan School* using *all but exclusively . . . lime-stucco or clay, sometimes burnt into terra-cotta* (p. 109), and separated by an intermediary period of about a century and a half (p. 112). This has been Sir John's theory for a long time. In 1949 Sir Mortimer

Wheeler opposed this view,¹ and there is no need to repeat here his sharp but, in my opinion, very accurate criticism. But the problem of materials may be considered.

Three materials other than stone are mentioned by Sir John: clay, terra-cotta, stucco (lime-stucco). Now, Sir John himself admits that the sculptors of the Early School were *doubtless familiar with clay and terra-cotta* (p. 109), though, he adds, *not a single figure . . . referable to the Early School has survived throughout the length or breadth of Gandhāra* (here opposed to Taxila). For clay this is but natural. Much less so for terra-cotta. But has architectural terra-cotta ever been used? I find this difficult to believe.² Thus one problem remains: stucco.

Surkh Kotal, although north of the Hindukush, may be adduced here as a parallel, especially interesting from its early date.³ Now, at Surkh Kotal, both clay and stone are used, and clay, let it be stressed, on a much larger scale than stone.⁴ But nowhere⁵ on this very large site, extensively excavated, has the smallest fragment of stucco ever been found; which is conclusive proof that stucco was not yet used, for sculpture in stucco is much less easily destroyed than sculpture in clay.

South of the Hindukush the situation must have been just the same; in early times sculpture was either in stone or in clay,⁶ then it became a habit to have the heads of clay figures made of stucco, the earliest instance of this technique being the heads from the Apsidal Temple at Sirkap (figs. 32–39). Later the stucco technique was extended to whole figures. Thus there is an early period of the school, known to us almost exclusively from stone fragments, the clay figures having perished; and a later period, known both from stone and stucco, or even mainly from stucco fragments. But the break supposed by Sir John to have existed between an *Early* and a *Later School* is an imaginary one, as Sir Mortimer rightly saw. It follows that Sir John's book is not what it pretends to be: *the story of the Early School*, but something rather different: the story of Gandhāra sculpture in stone. Let us add that Sir Mortimer was but one of the opponents to Sir John. A number of other scholars had expressed doubts, and Foucher himself, Sir John's great friend, who shared so many of his views, emphatically denied the existence of a hiatus between two supposedly different schools of sculpture, using two different kinds of material.⁷

The foundation for any story is chronology. Here three factors could be considered: dated inscriptions, stratigraphy, style.

¹ ANTIQUITY 1949, 17–18.

² In his *Taxila* publication Sir John mentions objects of clay fired by accident (*Taxila*, II, 470, nos. 175, 176, 181) and others intentionally, not accidentally fired (*ibid*, nos. 173, 174). Having seen the Taxila finds on the spot, I rather feel inclined to consider all the fragments of architectural reliefs looking like terra-cotta to be clay fired by accident.

³ The great sanctuary, the main monument, is a Kanishka foundation. The secondary temple (temple B) is a later addition. The Buddhist platform in the plain is later too. See ANTIQUITY 1959, 81–86 (with bibliography).

⁴ All the niches around the courtyard of the main temple were adorned with great figures of clay, brightly painted, fragments of which have been saved (our PLATE XXI(a); other instances in *Syria*, XXXVII, 1960, 1–2, pl. VI, 4, and VIII, 2). The altar of the secondary temple had its front side decorated with birds in an architectural setting, all in clay, *Journ. Asiat.*, CCXLII, 1954, p. 192, pl. IV, 1. The colossal statues on the Buddhist platform in the plain were of clay (our PLATE XXI(b); and *Journ. Asiat.*, CCXLIII, 1955, p. 276, pl. III, 1).

⁵ Neither in the Kanishka buildings, nor in the later monuments (temple B on the acropolis; the Buddhist platform in the plain).

⁶ It should be noted that some of the 'stucco figures' mentioned by excavators may be clay, see for instance J. Hackin, *L'oeuvre de la Délégation Archéologique Française en Afghanistan*, 1922–1933, Tokyo, 1933, fig. 1, given as *stuc*; but the description by A. Godard (p. 6) shows it to have been clay. Obviously it is exactly the same technique as at Surkh Kotal.

⁷ A. Foucher, *La vieille route de l'Inde de Bactres à Taxila*, Mémoires DAFA I, 1942–47, 346.

Dated inscriptions do exist, but, as is well known, the eras they use are not yet securely collated with the Christian era; several systems are being proposed. This being so, one can either decide for one of the systems, and say why, or consider the whole matter still unsettled.⁸ Sir John does neither. On the whole he disregards dated inscriptions to the point of seldom even mentioning one of them. But he may nevertheless, sometimes, take one into account. For instance, he chooses a certain piece for illustration in preference to another *because it bears on its base a Kharoshthi inscription dated in the year 87 of the Kanishka era. According to the now generally accepted chronology of the period, this date falls within a few years of A.D. 224, and it thus confirms the date of this sculpture arrived at on stylistic grounds* (p. 93). I cannot help feeling that the problem is not whether a certain chronology is *generally accepted*⁹ or not, but whether it is true, without any restriction or not. Until we are sure it is (which, in my opinion, we are not) nothing at all is being confirmed.

Stratigraphy we have nowhere, except at Sirkap, where it was built up by Sir John himself. Here it seems we could by all means expect it to be given precedence over style. Surprisingly this is not the case. For instance, discussing some toilet-trays from Sirkap (p. 17), Sir John will remark of one of them, considered by him to be the oldest specimen of the class, that, though *found in the fourth stratum (Early Śaka), it may well have been an heirloom from an earlier period*, the reason being, of course, that *its style is distinctively Hellenistic*; and of the next (p. 18), which *came from the fifth (Greek) stratum*, that it was *doubtless a stray from above and referable to the latter part of the last century B.C.*, i.e. to post-Greek times, the reason being that *the figures are very stiff and wooden by comparison with the preceding specimen*. Anyway, even for those inclined to treat Sir John's stratigraphy of Sirkap more respectfully than Sir John does himself, this stratigraphy is of little help for Gandhāra art. For, with the only exception of two small stūpas and of the Apsidal Temple, the ruins of Sirkap are non-Buddhist, which is but natural, Sirkap being a town; and the wealth of finds from monasteries around Taxila remain without stratigraphical connection with the finds from that town.

Style then is the real basis for Sir John's chronology. Now style is certainly not to be discarded as a means of building up chronologies. But if it is to be used at all, a necessary prerequisite is that some at least of the monuments should be securely dated for non-stylistic reasons. To any given province or period of archaeology these monuments will be what triangulation points are to a given district. Without them there will be no chronology, only a sequence of chronological surmises; just as without a triangulation there will be no map, only a tentative topographical sketch. As such monuments are lacking I can but consider the *story*, as told by Sir John, to be such a tentative arrangement, and a very conjectural one indeed.

Of Sir John's method let us give here at least one instance. The first of the two chapters on *Adolescence* begins with the *Mardān group*, a collection of sculptures about the *original provenance of which no reliable record seems to have been kept* (p. 40), although the majority are said to have come from Swāt. *Some characteristic examples* are then analysed (p. 41). *The first of these to merit our notice*, by which Sir John means the earliest, as all the context shows, is fig. 53 . . . *showing six figures disposed in a single row, and closely akin to those on several of the slabs discussed in Chapter 5. They are clearly carrying on the Hellenistic tradition. Hellenistic, too, are their poses and the treatment of their drapery, with its graceful folds and careful definition.* This is, quite obviously, the reason, the sole reason, why Sir

⁸ This is my position, v. *Syria*, xxxvii, 1960, 136.

⁹ An assertion which some people would, no doubt, emphatically reject.

SIR JOHN MARSHALL AND GANDHĀRA ART

John has put this relief first. It has been identified by Foucher as the Presentation of the Jetavana Garden. Among the six figures one recognizes *the Buddha distinguished by his halo*. This gives this relief *a unique value. It is the earliest representation of Buddha by a Gandhāra artist known to us*. Of course, I shall not deny the possibility of this being so. But is there any compelling reason to consider it an established fact? None, in my opinion.

Closely linked in age and style with the foregoing are two reliefs (figs. 54 and 55) depicting two other scenes of Buddha's Legend (p. 42). Closely linked in style they certainly are. Therefore they are likely—very likely, I agree—to be related in age too. Nevertheless this is but an inference.

The next relief (fig. 58) shows another scene of the same Legend. Similarities with the preceding reliefs are being stressed (p. 44). *But the workmanship . . . is not up to the same high standard* (p. 45), therefore it must be later.

Fig. 59, *the following piece* has much in common with fig. 54. *On the other hand, we cannot help being struck by the deterioration in the treatment of the robes. . . . But, while this panel evidences a further stage in the decline of the technical skill* (which is the reason for Sir John to place it here), *it also evidences a brave effort on the part of the sculptor to introduce a touch of realism, etc.*

The next of these reliefs, *in chronological sequence* are two panels . . . *doubtless issued from the same atelier* (p. 46). *Their close affinity to the sculptures already described is obvious. . . . But the carving is steadily becoming coarser, and losing little by little the sensitiveness of the earlier work; most of the actors, too, are of heavier, more thick set, build, etc. . . .* Therefore Sir John inserts it here.

Now even accepting without discussion Sir John's judgments on each of these reliefs, even seeing the differences between them exactly as he sees them, why could these not be due to different hands, for instance, or explained geographically? In other words, shall we really feel compelled to follow Sir John and to explain them chronologically? I do not think so.

Should we nevertheless accept *the story* as reconstructed by Sir John, at least partially and for the time being, as a working hypothesis? Let it be said that such a cautious position would not be in accordance with the views of the author himself. Chapter 12, the chapter of conclusion, opens with the following astonishing sentences: *Such, in rough outline, is the story of the Early School of Buddhist art in Gandhāra, as revealed by my discoveries at Taxila. Now that we know the truth* (the underlining is mine) *and can follow, step by step, the course of its evolution, we may wonder why earlier efforts at its reconstruction fell so wide of the mark.*

I believe we are still far from 'knowing the truth' about 'the story'. Why did Sir John not succeed in building it up? Simply because chronological data cannot be replaced by stylistic considerations. Any attempt at describing a historical development when such data are still so completely lacking is, it seems to me, a hopeless undertaking. Where Sir John has failed everybody would have failed as well.

Should we just say, then, that times are not yet ripe for any general book to be written on Gandhāra art? This, in my opinion, would be going too far. The grave doubts expressed here are on the possibility of writing such a book as 'a story', not on the possibility of attempting a general classification of Gandhāra sculpture, a task that the actual wide dispersion of the fragments renders all the more desirable. Foucher had arranged this mass of material according to iconography. It remains to be arranged according to petrography, to geography, and to style. The fragments could be grouped from the kind of stone they are made of, and from provenance (whenever this is known), two secure

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bases; then from style, a basis much less secure, but none the less important; the ultimate problem remains to decide whether or not such a classification would allow some historical conclusions to be drawn, at least tentatively.

For such a book much valuable material is to be found scattered in the present book. Factual data known to Sir John are scrupulously given, and here, of course, his long familiarity with the matter makes his testimony irreplaceable. This profound knowledge also results in some excellent parallels, as, for instance, p. 34, when he recognizes on a Gandhāra relief (fig. 40) a certain type of goblet (fig. 42) known in numerous specimens from the Parthian stratum of his excavations at Taxila.

In conclusion, two remarks may be of use.

First, Sir John has written his book from the monuments. Foucher excepted, whom he constantly quotes, all the work done by so many scholars on the subject is scarcely considered. This is certainly better than to have taken much notice of other people's opinions and little notice of the monuments. Nevertheless, that the 'Romano-Buddhist' trend of ideas should not be taken into account, save for one casual mention (p. 106) of that great forerunner, Vincent Smith; that the opinions of B. Rowland, whose first article on the subject goes back to 1936, of Buchthal, of Mortimer Wheeler and others, should be entirely ignored is somewhat surprising.

Secondly, the monuments shown and commented on are the ones Sir John knew in British India, the only exceptions being a few pieces at the British Museum (figs. 41, 47, 48, 121, 129, 130), one at the Victoria and Albert (fig. 95), one at the Musée Guimet (fig. 49), one at the Kabul Museum (fig. 50). This again is certainly much better than the reverse. Nevertheless, the fact that the Gandhāra fragments in Europe and in America are so little considered, that but three pieces of Buddhist sculpture from Afghanistan are shown (figs. 49, 50, 121, all three from Hadda) should be remembered.

Any future attempt at a balanced picture of Gandhāra art should I believe, pay more attention to predecessors' opinions as well as to Gandhāra pieces outside Pakistan and India.

Book Chronicle

We include here books which have been received for review, or books of importance not received for review, of which we have recently been informed. We welcome information about books, particularly in languages other than English and American, of interest to readers of ANTIQUITY. The listing of a book in this chronicle does not preclude its review in ANTIQUITY.

ARCHAEOLOGY IN THE U.S.S.R. By A. L. MONGAIT. pp. 320 with 30 text figures and 24 plates. London, Penguin Books, 1961. 5s. A translation by M. W. Thompson of the original Russian book *Arkheologiya v S.S.S.R.* published in 1955 with a special preface by the author for the English Edition.

ENGLISH MONASTERIES IN THE MIDDLE AGES. By G. H. COOK. pp. 282 with 41 text-figure plans and 32 plates. London, Phoenix House, 1961. 36s.

ADOMNAN'S LIFE OF COLUMBA. Edited with Translation and Notes by the late A. O. ANDERSON and M. O. ANDERSON. pp. 590. Edinburgh, Nelson, 1961. 50s.

VOICES IN STONE. By ERNST DOBLHOFFER. pp. 327, 16 plates, 94 text figures. London, The Souvenir Press, 1961. 30s. (A translation of the book *Zeichen und Wunder* first published in 1957 by Paul Neff Verlag in Vienna.)

[continued on p. 256.]

Prehistoric Salt-Boiling

by KARL RIEHM

Dr Karl Riehm is a doctor of medicine at Halle-Giebichenstein, who has for the last ten years been working in cooperation with the Landesmuseum für Vorgeschichte on problems of prehistoric salt-mining and production. He recently received the Leibniz medal of the Deutschen Akademie der Wissenschaften, Berlin, for this work. He here discusses the technique of the salt-boilers of prehistory, with special reference to our knowledge of briquetage, and to our understanding of the Red Hills of the Essex coast.

I. THE RED HILLS OF THE ENGLISH COAST

Just over fifty years ago the puzzling Red Hills on the coast of Essex were arousing considerable interest among English prehistorians (PLATE XXII (a) and (b)). Already in the spring of 1906, under the chairmanship of J. Chalkley Gould and supported by the Essex Field Club, the Society of Antiquaries of London had formed a 'Red Hills Exploration Committee', which counted among its members not only archaeologists but also geologists, botanists and chemists, and had ample means at its disposal. Eminent specialists and scholars examined suitable hills with the utmost care and published their findings and conclusions in comprehensive papers.¹

But in what consists the peculiarity and the riddle of these Red Hills, which obviously date back to prehistoric times and have since time immemorial lain in close proximity to the coast of England? The striking way in which they are built of loose red burnt clay unmistakably points to a human-made origin. As a rule these mounds rise by only as much as 18 in. to 6 ft. above their surroundings, and their extent varies between a few square yards to several hundred. How did these numerous and extensive heaps of burnt clay come about in prehistoric times?

A further riddle about these hills are the peculiar objects of burnt clay, fragments of which are found in varying numbers within them, often embedded in layers of ash and charcoal. Thousands of similar finds have been known from the upper reaches of the Seille valley in Lorraine, which is rich in salt springs, and where, since Keune's diggings, they have been connected with the gaining of salt from salt wells in prehistory.² Of the various unusual objects of this kind, in Essex as well as in Lorraine, particularly striking are bar-like fragments, round, square or rectangular in section, made of a mixture of clay and chaff, while the multiform connecting pieces or lutings do not show this addition of perishable material to the clay at either place of discovery. Among further typical component parts of this group of finds in both places we wish to mention as important variously-shaped porous clay containers, as well as numerous sherds of large cooking

¹ F. W. Reader (1907-9, 1909-11), Excavation Reports of 29 March, 1908, and 17 February, 1910, to the Red Hills Exploration Committee, *Proc. Soc. Antiqu. London*, II Ser., 22, 164-207 and 23, 69-96.

² I. B. Keune (1901), 'Das Briquetage im oberen Seilletal', *Jahrb. Ges. lothr. Gesch. Altertumsk.* 13, 360-394.

pots. According to the linguistic usage of French scholars, who first described these finds on the Seille as early as 1740, the scientific designation of *briquetage* has become customary for this complex of finds.

'Red Hills' are found very frequently on the coast of Essex, where several hundred of them have been counted. But also in the counties north of Essex these mounds are no rarity—always situated in the vicinity of the sea or of the mouth of a river. There are also sites on the south and west coast of England (Kent, Sussex, Hampshire, Dorset, Somerset), as well as in the Channel Islands, where finds similar to those of the Red Hills are encountered, though fewer in number. In other parts of Europe, too, numerous places containing *briquetage* have been discovered, as, for example, on the Atlantic coast of France, especially in southern Brittany, on the Dutch-Belgian coast, as well as on the Caucasian Black Sea coast. There are a few sites also in the interior of Germany, where occur great quantities of *briquetage* or similar shaped strange objects of clay. This is the case especially in central Germany in the region of the ancient salt town Halle on Saale. Other important places of *briquetage* in Germany are Schwäbisch Hall and Bad Nauheim.³

Let us return once more to the Red Hills Exploration Committee. It was in the first place F. W. Reader and H. Wilmer who were actively engaged in the exploring of the Red Hills of Essex and who extended their comparative researches to include Brittany. Comprehensive reports and excellent maps bear witness of their ample and various finds and the conclusions arrived at. Even today they convey to us a vivid picture of the connection established by them between the Red Hills, their *briquetage* and the fabrication of salt in prehistoric times.

It is sad to reflect that this joint labour of research should after all have been denied the hoped-for solution of the Red Hills mystery as the deserved reward of all the effort expended. For all the valuable partial knowledge and insight gained, no convincing explanation of the origin of the Red Hills and of the purpose of the *briquetage* had so far been forthcoming. As early as March 1908, on behalf of the Committee, H. Wilmer openly expressed disappointment with this state of affairs: '... the Committee were disappointed not to have attained decisive results after two years active work'.⁴

Although the first diggings had led to no results, the two scholars did not cease to take their research task seriously and to feel responsible to the Committee; they went so far as to seek personal contact with the *briquetage* experts of Lorraine, like Keune in Metz and the Count Beaupré in Nancy.⁵ But even this visit failed to bring the desired progress, either in respect of the mysterious genesis of the Red Hills or the significance of the *briquetage* contained in them. A detailed study of the Seille finds rightly led them to reject, in the case of the English *briquetage*, the idea of the Lorraine investigators of a heated framework of clay bars over which salt water had been made to drip in order thus to grade and crystallize the salt.

After a period of ten years R. A. Smith⁶ took up the question, thus summarizing and concluding it: 'When careful excavation extending over four seasons has left the Red Hills of Essex unexplained, *a priori* reasoning may be adopted as an auxiliary method of inquiry; and the failure of a local committee to solve the problem prompts one to look at the facts from outside, and from Continental experience to decide what ought to be found in the positions so carefully located on the map. Similar archaeological material has been

³ K. Riehm (1954), 'Vorgeschichtliche Salzgewinnung an Saale und Seille', *Jahresschr. mitteldt. Vorgesch.* 38, 112-156.

⁴ F. W. Reader (1907-9), *op. cit.* 188.

⁵ R. A. Smith (1918), 'The Essex Red Hills as Saltworks', *Proc. Soc. Antiqu. London*, 11 Ser., 30, 53.

⁶ *Ibid.*, 36-37.

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found on several occasions, and the constant presence of at least one factor has a cumulative effect that may override objections based on minor differences. As a member of the Red Hills Exploration Committee I feel that full weight has not been given to cognate discoveries elsewhere, and therefore offer to this Society, which published the original reports, a series of extracts mostly made some years ago from foreign publications that might easily be overlooked by those still interested in the Red Hills problem.⁷

R. A. Smith submitted a series of reports on *briquetage* sites on the Continent, which confirmed his view that the Red Hills fragments should be interpreted as the remnants of an extensive salt gaining industry of pre-Roman times. He concluded by offering his very own detailed suggestion of a reconstruction of the forgotten technique of the ancient salt boilers; this interpretation points to new ways, especially in the exact description of prehistoric basins of evaporation; in the end, however, Smith closely approximated his interpretation to that of the Lorraine scholars.⁸

When discussing the Red Hills problem, R. A. Smith characteristically said: '... as no conclusive evidence is forthcoming from Britain, the proof of these contentions has to be sought abroad', by means of analogous finds and experiments on the Continent.⁹

2. NEW BRIQUETAGE AT HALLE AND NEW ATTEMPTS AT INTERPRETATION

Since 1825 the *briquetage* found in great quantity at Halle on Saale has been the subject of investigation. It had, however, so far not been possible to furnish any satisfactory explanation as to the purpose of the strange and multiform shapes of clay objects.⁹ Cylindrical fragments, so-called *Tonstützen* (clay supports), are especially frequent there. Also in the wider neighbourhood of Halle, where a substratum is formed by the mighty salt layers of the Permian Sea, more than a hundred *briquetage* sites have been discovered.

In the summer of 1955, when a building site was being excavated for the erection of a new university department on the left bank of the river Saale, broken chalice-like objects with a pillar-like clumsy foot were met in great numbers.¹⁰ One of these goblet-like vessels was so little damaged that it could easily be restored to its original state (FIG. 1 (a)). The discovery in the *briquetage* of Halle of this type of goblet, unknown so far, shed new light on the destination of the enigmatic utensils, and extensive investigations and attempts at gaining salt were made by the museum of prehistory (Landesmuseum für Vorgeschichte) at Halle, with the aid of copies of the original chalices and using the natural brine of the springs at Halle.

These highly porous objects proved unsuitable as boiling vessels. For when the brine was poured in them the clay sucked up the liquid like a sponge, and on heating the walls burst so that the brine flowed out. On the other hand, it was found possible in the case of largish impermeable vessels—as they, too, were found at Halle—to evaporate and concentrate brine by constant additions of small quantities during the heating until loose crystallized salt sank to the bottom of the vessel.

Under these circumstances it was tempting to assume that the chalices of Halle were salt cake moulds and parts of salt drying apparatus. Filled with the freshly boiled salt obtained from the containers, they probably stood around a glowing hearth. This mould was, however, suitable for one use only: it had to be smashed when the hardened salt cake

⁷ *Ibid.*, 50–51.

⁸ *Ibid.*, 46–47.

⁹ K. Riehm (1958), 'Neue Einblicke in die Technik der vorgeschichtlichen Salzsiedekunst', *Forschungen und Fortschritte*, 32, 47–49.

¹⁰ K. Riehm (1959), 'Genormte Tonbehälter zur Formsaltzfertigung in der Vorzeit', *Ausgrabungen und Funde*, 4, pl. 1.

was taken out. The best temperature for gaining dry salt was found to be a mild irradiation of only 60° to 70° C. When intensive heat was used, violent evaporation of the salt set in, forming blisters and loosening it. This led to the view that the prehistoric saltboilers were aiming at giving the freshly boiled loose salt a firm shape of a definite size, i.e. producing lumps of hard salt of as equal a weight as possible, by making use of these vessels in the process of drying. It stands to reason that the great advantage for bartering purposes in

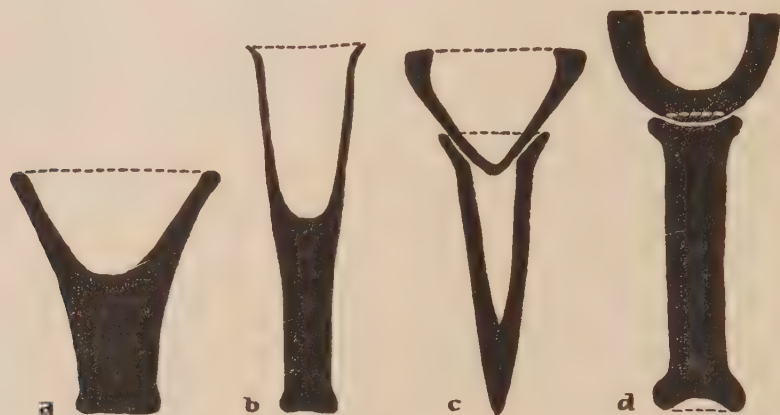


Fig. 1. Development of the salt cake moulds of Halle. (a) Goblet, (b) Narrow Chalice, (c) Hollow Cone on Quiver, (d) Jar on a cylindrical pillar. Scale 1:3.

using salt cakes of an equal size amply justified the increased labour involved in shaping the salt. For in commerce every single whole piece of salt now represented a certain unit of this vital aliment, condiment, and preserving agent. Thus, in the absence of balances and weights, handily shaped pieces of salt could easily be used as a unit of value between the bartering parties. As a rule the salt cakes were probably marketed together with their 'packaging', i.e. in their mantle of clay. The pillar-shaped foot of the chalice had to be knocked off before trading.

3. THE DEVELOPMENT OF THE PREHISTORIC SALT MOULDS OF HALLE ON SAALE

Stratigraphical observations have permitted us to classify the manifold types of *briquetage* of Halle in sub-groups of the same periods. It was thus possible in the end clearly to distinguish four stages of development in the processes of shaping and drying the salt. These clearly point to the economic progress shown in each subsequent working method as compared with the preceding one (FIG. 1 (a)–(d)).

The *goblets* of Halle (FIG. 1 (a)) were probably in use at the end of the Bronze age—i.e. from about 1000 B.C. Also their successors, the much more frequent and widely-spread champagne glass-shaped narrow *chalices* (FIG. 1 (b)), must in all likelihood be dated to the end of the Bronze age. Like the goblets, they were mass produced in series; a carved wooden model was covered with plastic clay. As a result of this kind of mass production the volume of all of them was identical in shape and size, being taken from the same gauge. Thanks to their uniformity and great numbers, the salt moulds of Halle were soon assigned the role of monetary units in barter. One may even go so far as to speak of a salt currency of those ancient times. For these salt cakes were unhesitatingly accepted as stable currency

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by the bartering parties even where they were not being bought for consumption; the valuable salt could at any time be exchanged for other kinds of merchandise. In order to make transportation easier the walls of the narrow chalices were made rather thin to reduce the weight. Compared with the goblets they took up less space around the hearth during the heating process. Another advantage was the handy format of their quasi-cylindrical salt cakes.

A further step forward in the development of the prehistoric salt moulds of Halle was the clever division of the unit into two, i.e. a hollow cone with thin walls (FIG. 1 (c), upper part), being the salt mould proper and a quiver-like support. Now it was no longer necessary when making a salt cake to throw away as useless the pillar-like foot. For the new quiver (FIG. 1 (c), lower part) stuck into a tubular opening at the bottom of the hearth could be used again and again to support a salt mould.

More perfect than this pair of utensils, which was soon to be superseded, was the subsequent *cylinder pillar* with the thick-walled *hemispherical jar* (FIG. 1 (d)) resting on it. As a salt mould this was very advantageous economically in that it made the stage of firing the clay superfluous. All that was necessary was shaping over a gauge. The great porosity and the thick walls favoured the absorption of the remaining mother-liquor. The brittleness of the unfired clay cover made it easy to remove the salt cake.

The new interpretation of the significance of the *briquetage* vessels of Halle, namely, as earthen moulds for the manufacture of hard salt cakes by the heat of the hearth, can equally be applied to the puzzling finds of all the other known *briquetage* sites of Europe. Their amenableness to interpretation is reflected by the ease with which we can now explain the use made of certain finds that had hitherto defied explanation.¹¹

It should be pointed out that in nearly all *briquetage* sites in Europe three groups of finds can be distinguished: (1) large boiling pots; (2) porous salt moulds; and (3) supports to the latter. An exception to this rule are the stone chambers of Mesquer (Brittany), where instead of the clay supports we find granite slabs. In this rough scheme there are, of course, many local variations. On some of the sites additional tools and vessels have been found.

4. THE MOST IMPORTANT SITES FOR PRODUCTION OF SALT IN ENGLAND

In England, too, the local *briquetage* sites have been investigated from an early date. In two publications of the year 1850, four scholars expressed their views: W. A. Nicholson,¹² Albert Way,¹³ A. W. Franks,¹² and above all Frederick C. Lukis,^{12, 13} endeavoured to solve the question of the significance of the roughly-cylindrical 'handbricks' found on the Lincolnshire coast and in the Channel Islands. Of the various *briquetage* sites described in the literature the English coast shows three special regions with finds that seem to be largely open to interpretation on the basis of the salt-mould hypothesis. These are the Isle of Purbeck and the coasts of Lincolnshire and of Essex.

In Kimmeridge it was above all the half-cylinders which puzzled J. B. Calkin.¹⁴ The strange thing about these hollow cylinders, which are from 3-4 inches wide, is an incision made before firing on either side of the vertical outer wall and across the bottom. This deep incision divided the cylinder into connected halves (FIG. 2 (a)). A slight rap on these lines sufficed to break the vessel into two equal parts and to release the dried hardened

¹¹ K. Riehm (1960), 'Die Formsalzproduktion der vorgeschichtlichen Salzsiedestätten Europas', *Jahresschr. mitteldt. Vorgesch.* 44, 199.

¹² *Archaeol. Journal*, VII (1850), 69-70.

¹³ *Ibid.*, 175-176.

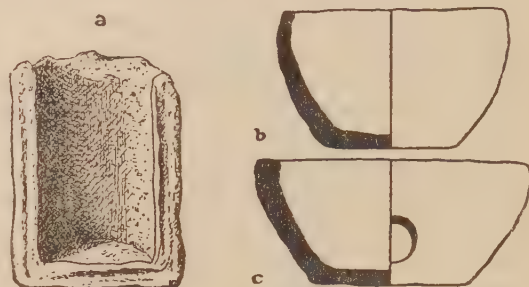
¹⁴ J. B. Calkin (1948), 'The Isle of Purbeck in the Iron Age', *Proc. Dorset Nat. Hist. and Archaeol. Soc.*, 68, 56-57.

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salt cake intact from its clay casing. This is done as it had to be flawless when offered to the merchant and used for barter. If the saltboilers had done without the pre-formed line of incision on the cylinders there would have existed a great danger of breaking the salt cake in the process of removing it from the mould.

The hard round bowls, similar in shape to sugar bowls and found not too far from the first place of discovery, also seem to have served as salt-cake moulds (FIGS. 2 (b) and (c)).

Fig. 2. Salt cake moulds of Kimmeridge.
(a) Half Cylinder, (b, c) Bowls (after Calkin). Scale 1:4.



They are probably of a more recent date and could be used repeatedly. An inch-size hole in the side wall at a level with the bottom observed in some examples was probably meant to drain off the surplus mother-liquor from the damp salt that had been poured in. This little device helped to save firing material.

The technique of the iron-age saltboilers of the coast of Lincolnshire, described to us by Swinnerton from the region of Ingoldmells,¹⁵ seems to have been rather advanced. The very variety of *briquetage* types here is surprising. Despite the varied character of the material found, we gain the impression that all the vessels belong to the same period. From the point of view of the salt-mould hypothesis they can easily be classed in a technically suggestive order.

The Lincolnshire *briquetage* can as a rule be found in the Red Mounds—nowadays low and not very extensive elevations in the immediate vicinity of the beach—especially after they have been swept clean by high tides. Here, underneath ashes and loose burnt clay, Swinnerton found flat, rectangular fireplaces with a base of loam as well as fragments of all sorts of *briquetage* (FIG. 3 (a)). This is not the place to discuss in detail the circumstances of the discovery nor the description of the various utensils of which the discoverer gave a careful and comprehensive account.

The manifold remains permit us to reconstruct in the main the working processes of the ancient saltboilers. In the spring the sea brine was permitted to run into flat sunpans (possibly with a light roof) and the supply trench was cut off. The liquid was left to evaporate during the summer months. In the autumn the remaining crust of salt and the underlying layer of clay containing a great amount of salt were scraped off the dry floor of the basin and the material thus collected thrown into an open fire.¹⁶ Even today such firemarks can be recognized in the old sunpans. Through the intense heating the clay, which had been grey, ferrous and extremely finely granulated, now changed into a red coarsely granulated mass.¹⁷ The salt clay, which was now sand-like, was put into a

¹⁵ H. H. Swinnerton (1932), 'The Prehistoric Pottery Sites of the Lincolnshire Coast', *Antiqu. J.*, 12, 239-253.

¹⁶ K. Riehm (1959), Die Red Hills der englischen Küste und ihre Problematik', *Jahresschr. mitteldt. Vorgesch.*, 43, 235-238.

¹⁷ R. A. Smith (1918), *op. cit.*, 50-51.

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container and brine was added until all the salt present was taken up in a concentrated solution. Had the clay not been roasted prior to being diluted in water, a milky loam broth would have resulted in which the salt solution could but very imperfectly have been separated from the smeary clay mush. A great deal of salt would thus have been lost. Only the roasting of the salt clay made it possible to separate cleanly the salt from the clay. The saturated salt solution was poured off the sediment while the red dry residue was thrown away.

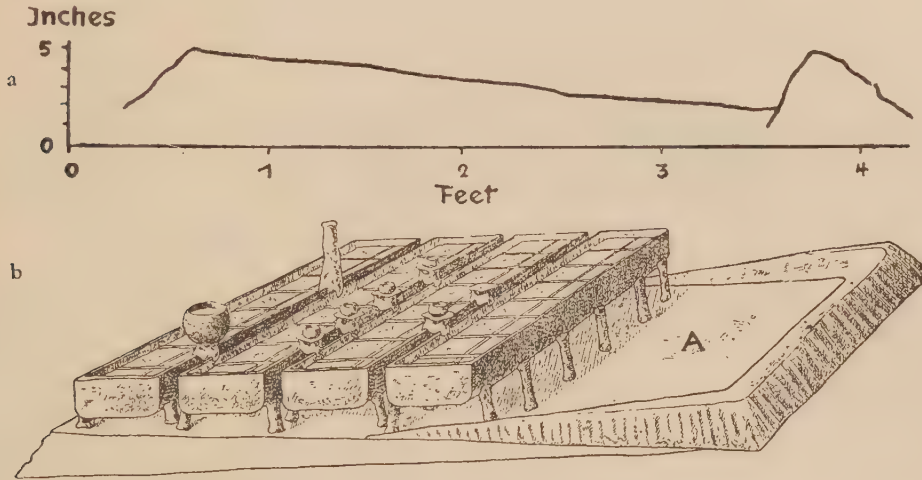


Fig. 3. (a) Vertical section of a salt-boiling oven of Ingoldmells, Lincolnshire (after Swinnerton); (b) Reconstruction of a salt-boiling oven as described by Swinnerton. A = Hearth and site of boiling vessels.

The liquor gained was put on the hearth in large vessels for boiling. When after several pourings a sufficient amount of salt had settled in the vessel, the surplus mother-liquor was poured away and the strongly porous rectangular dishes were filled with the crystalline salt. These dishes were about 60 cm. long and were held up by several handbrick-like supports. The round insets,¹⁸ which Swinnerton noticed on their inner side, allow the conclusion that partition walls made of clay¹⁹ were inserted in the dishes fixed by means of soft clay balls which were placed length-wise and across the dishes. They served to subdivide each dish into many single compartments, thus determining the shape and size of the salt cakes to be manufactured. In order to make them uniform and true to gauge, cubelike pieces of burnt clay²⁰ were used, which were placed into the dish before the heating process. These partition walls were leaned against the cubes, fixed to the wall and the cubes taken out before the loose salt was poured in. Swinnerton informs us of the dimensions of two carefully worked clay cubes which were found by the same hearth. Thus we are able to calculate their volume. The larger one measured $8 \times 4\frac{1}{2} \times 4\frac{1}{2}$ cm. = 162 c.cm., the smaller $5 \times 4 \times 4$ cm. = 80 c.cm. This doubling of their volume does not seem to be accidental. Rather is it probable that single and double pieces of salt cakes were manufactured. Even the heat rising in the interstices between the closely

¹⁸ H. H. Swinnerton (1932), *op. cit.*, 246

¹⁹ *Ibid.*, 246.

²⁰ *Ibid.*, 248, fig. 9, nr. 7.

packed dishes was utilized; numerous bands of soft clay²¹ were put on the rims of the adjacent dishes; on these were stood pygmy-bowls filled with salt of a spherical shape and a height of only $2\frac{1}{2}$ cm.²² The salt cakes made in these obviously represented the 'small change' of the saltboilers (FIG. 3(b)). Less often were found somewhat larger salt moulds which resembled round bowls. F. T. Baker²³ explored in 1953 a *briquetage* site south of Ingoldmells; this dates from the Belgic period (first half of the 1st century A.D.): the technique barely differs from that of the older production period.



Fig. 4. Triangular wedges from the Essex Red Hill. Scale 1:4.

It is likely that a working process similar to the one in Lincolnshire was customary also in the coastal workshops of Essex related to the *briquetage* material in the Red Hills. The porous saggars found here were obviously also used as dry containers for the production of moulded salt. This would be in keeping with the fact that these saggars not infrequently show an inch-size round hole in their side wall at a level with the bottom, destined for the drainage of the mother-liquor. However, in place of the vertical hand-bricks, here horizontal grates of fire-bars above the earth seem to have been customary, at least at some times. The peculiar triangular wedges possibly subdivided the salt-filled saggars, the broad obtuse wedges forming the longitudinal ribs and the narrow acute wedges the transverse ribs of the grate (FIG. 4). On the inner wall of a sagger fragment is a button-shaped protuberance with a vertical lath, against which at one time a wedge or a sheet was probably leaned. In later times—but probably still before the Roman occupation the drying of the salt seems to have been undertaken by placing the filled vessels into special flues.²⁴ It can be inferred from the scanty settlers' tools and rubbish heaps in the Red Mounds and Red Hills that the coastal workshops of the ancient saltboilers were only seasonally occupied.

5. DESCRIPTION OF SOME SITES ON THE COASTS OF HOLLAND, BELGIUM, FRANCE AND THE BLACK SEA

Let us now turn to other *briquetage* sites on the Continent. In Holland on the Bosch- and Gasthuizer Polder near Leiden, we encounter strange cleft cylinders together with small handbrick-like supports and large four-edged bars dug out by Modderman in 1956. These hollow cylinders are usually found as half-cylinders whose one vertical edge is broken while the other is cut with a knife prior to firing. The diameter of the cylinders is about

²¹ *Ibid.*, 250, fig. 9, nr. 9(a).

²² *Ibid.*, 244, fig. 6.

²³ F. T. Baker (1959-60), 'The Iron Age Salt Industry in Lincolnshire', *Lincolnsh. Architectural and Archaeological Society*, vol. 8, New Series, 26-34.

²⁴ F. W. Reader (1907-9), *op. cit.*, 75; H. Wilmer (1907-9), 'Late Celtic Remains on the Coast of Brittany comparable with the Red Hills', *Proc. Soc. Antiqu. London*, II Ser., 32, 207-214; R. A. Smith (1918), *op. cit.*, 49 footnote; C. F. C. Hawkes (1933), 'Early Settlement at Runcion Holme', *Proc. Prehist. Soc. East Anglia*, VII, 258-262.

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4 cm., whereas their height, owing to the fractures, cannot be determined from the fragments. The narrow vertical cleft—according to the discoverer, who is familiar with the new idea of the salt moulds—has a dual purpose: the draining of the mother-liquor and the easy breaking of the mould by pulling it apart with the fingers of both hands placed inside the cleft. Thus there was no danger of the salt cake being broken.

The best-known *briquetage* site in Belgium is La Panne, close to the French border. It shows impressively how well the prehistoric saltboilers knew how to adapt their technique to the geological formation of the soil. The sandy beach was ill made to hold up pillar-like supports with flat bases as supports of the salt moulds, thus we find here as supports of the earthen salt moulds about 18 cm. high supports like nails, the so-called *spijkers*. Porous round bowls filled with salt of a height of about 9 cm. whose uniformity is stressed by the excavator Rahir were, with the aid of flattened clay balls, the so-called 'clay pastilles', attached to the flat head of a nail, which was stuck into the sand at the hearth. The clay bowls, whose walls were only 0.3–0.5 cm. thick, were broken when the dry salt was removed.²⁵ The other types of *briquetage* at La Panne cannot be discussed here. (Brief mention, however, must be made of the site of Étaples, a small fishing port about 25 km. south of Boulogne-sur-mer, which was discovered as late as 1959 by H. Mariette.)

Recently, Nenquin,²⁶ a Belgian specialist in these problems who has also made a study of English *briquetage*, referred to mass finds of rectangular and thin-walled small dishes on the south-west coast of Brittany, quoting two publications by Y. Coppens. The latter draws attention to forty-two sites alone in the Morbihan. The proportions of the porous containers are: height 4–5 cm., length at the top 7 cm., width at the top 4 cm., length of the bottom 5 cm., width of the bottom 2 cm. Their shape resembles a flattened pyramid standing on its head. By analogy with the *briquetage* of Halle, Nenquin interprets these fragments as salt cake moulds of the Celtic population.

Briquetage was recently also discovered on the shore of the Pointe-Saint-Gildas projecting into the sea south of the mouth of the River Loire. There were found baked clay fragments of many small cylinders about 20 cm. high, cuplike in shape on the top and very many fragments of porous containers. Small lumps of clay indicate that here also, as at Nalliers, the forms for the salt were fixed with lumps of soft clay on top of the pillars.²⁷

The main *briquetage* sites of the Vendée extend seaward of Nalliers. They are described by L. de Fleury.²⁸ Here we find the same small dishes as in Brittany (FIG. 5 (c)). For this region, however, a three-armed round support is characteristic (FIG. 5(a)). Less frequent but having been in use at the same period are the andiron-like 'arcade supports' (FIGS. 5(b1), and (b2)). Nearly identical twin supports are known from Halle and its surroundings. They bore pairs of rather large oblong salt moulds on their edge. Thus, both these shapes of supports from L'Ileau des Vases suggest that at one time at least two value units of salt cakes were manufactured there. In the neighbourhood of some *briquetage* sites on the French coast indications of basins for evaporating the water of the sea are also known.

²⁵ E. Rahir (1927), 'L'âge du fer à La Panne, une fabrique de poteries', *Bull. Soc. Anthropol. Bruxelles*, 42, 21–35; A. de Loë (1906), 'Fouilles à La Panne', *Bull. Mus. Roy. Arts décoratifs et industriels*, 6, 3–4; (1908) 11 Ser., 1, 36.

²⁶ J. A. E. Nenquin (1955), 'Zoutontginning in de Praehistorie', *Handelingen des Maatschappij voor Geschiedenis en Oudheilkunde*, Nieuwe Reeks 9, 4–16; J. A. E. Nenquin (1960), 'The relations between Southern Britain and Continental Europe during prehistoric times with special reference to salt-production', Ph.D. Dissertation, Cambridge.

²⁷ M. Tessier (1960), 'Découverte de gisements préhistoriques aux environs de la Pointe-Saint-Gildas', *Bull. de la Soc. préhist. franc.* 57, 428–434.

²⁸ L. de Fleury (1888), 'Dépôts de Cendres de Nalliers (Vendée)', *Rev. Archéol.*, 3 Ser., 349–357.

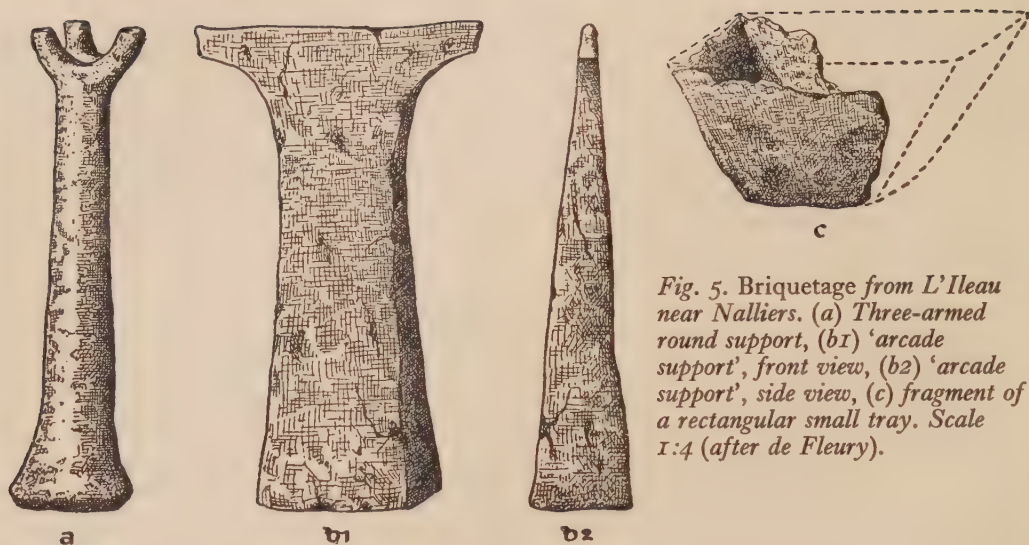


Fig. 5. Briquetage from L'Ileau near Nalliers. (a) Three-armed round support, (b1) 'arcade support', front view, (b2) 'arcade support', side view, (c) fragment of a rectangular small tray. Scale 1:4 (after de Fleury).

The *briquetage* sites on the Caucasian Black Sea coast, described by L. N. Solovjev, are grouped around the resort of Suchumi. Owing to the gravelly sand beach, we here find, just as at La Panne, clay supports with pointed feet.²⁹ The rectangular salt moulds of a height of 10–12 cm. and a circumference of 40–25 and 30–20 cm., respectively, are manufactured serially by placing a cloth into a hollowed wooden mould and pressing a thin layer of plastic clay on it (FIG. 6). Thus we can still today study the exact details of the weaving technique practised at the time ('textile ceramics') from the imprints left on the salt mould fragments.

6. CHARACTERISTICS OF SOME SITES NEAR INLAND SALT SPRINGS IN FRANCE AND GERMANY

Keune, in 1901, suggested that the ancient saltboilers on the river Seille had erected a heatable scaffolding of a height of about $1\frac{1}{2}$ m. and made of bars of clay, surmounted by a large brine vessel. The brine dripped over the hot bars on which the salt crystallized. It was then harvested through scratching. In 1913 he withdrew this interpretation. He was now of the opinion that the salt was gained through boiling the brine, and that the clay bars were the building material of a hearth with a grate, on which lay oval sheets of clay bearing the boiling pots.³⁰

Little is known of the new finds discovered by Harter in 1937 in Marsal.³¹ Here we are dealing with rather thin cylindrical staves of a height of about 12–15 cm., which stood in a three- or four-edged base. On their points was mounted a small thin-walled beaker of a height of about 8–10 cm., borne by a clay lump or a superimposed second clay base. The salt cakes made in these beakers, well protected in their casing of clay, were traded in that form.

²⁹ L. N. Solovjev (1950), 'Selisce s tekstil'noj keramikoju na probereze zapadnoj Gruzii', *Sovjetskaja Archaologija*, 14, 276–282.

³⁰ J. B. Keune (1913), *Lothringen und seine Hauptstadt Metz*, 45 ff.

³¹ I. Harter (1938), *Étude monographique sur le Briquetage de la Seille*, MS of a paper read at the *Congrès Lorrain des Sociétés Savantes de l'Est de la France* at Nancy, 6–8 June, 1938.

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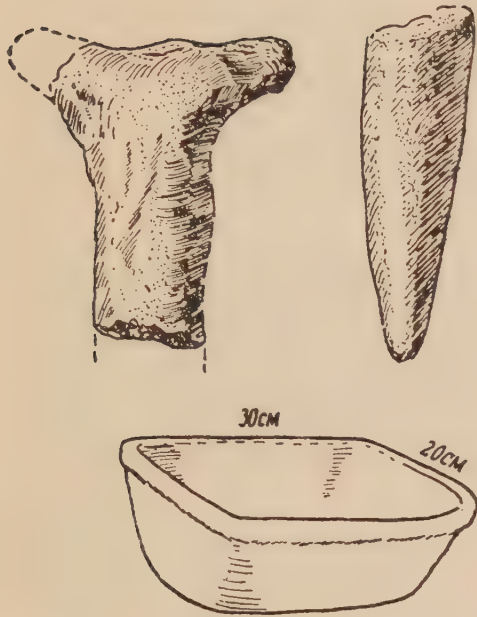


Fig. 6. Horned nail support and rectangular bowl from the Causasian Black Sea Coast (after Solovjev).

The Celtic saltboilers of Bad Nauheim in Germany of the late La Tène period, however, used conical-round thick-walled and hard-burnt goblets of a height of 18–20 cm., which were presumably destined for repeated use as salt moulds. Space does not permit to do more than mention the *briquetage* of Schwäbisch Hall, of the salt wells of Frankenhausen and of the region of the erstwhile ‘Salty Lake’ near Eisleben.

7. THE SALT-GAINING TECHNIQUE OF THE NATIVES OF AFRICA

Let us in conclusion cast a short glance at the salt-gaining methods of the natives of countries with a primitive technique and compare them with the working methods of prehistoric man. Of the observations and accounts of African explorers it was above all Springer who gave us many good illustrations.³² The saltboilers of those places used seawater, brine, rock-salt and vegetable ashes as their raw material. Nearly without exception and for purposes of barter the boiled salt is eventually given a firm shape of a definite size, sometimes with the use of special gauging vessels. Drying the salt in clay moulds near the hearth is unnecessary in the heat of Africa. But even with drying in the sun, clay and wooden moulds are sometimes used (PLATE XXIII (a)). More often do we find tubes of matting, especially cylindrical containers made of large leaves, often covered with bast or a mixture of clay and cow-dung.

Explorers sometimes also encountered salt-boiling ovens, in which the brine was completely evaporated in burnt clay moulds having an interior made impermeable and with the brine being poured over them repeatedly (PLATE XXIII (b)). These are marketed in their containers. Near the rock-salt works the explorers regularly found high rubbish heaps similar to those of the Red Hills. The evaporation basins on the coast of Upper Guinea also resemble those of the Essex coast. Here, too, the remaining crust of salt is scraped together with a part of the earth and then diluted in hot brine.

³² A. Springer (1918), *Die Salzversorgung der Eingeborenen Afrikas vor der neuzeitlichen europäischen Kolonisation*, Inaugural-Dissertation Jena.

Early Bronze Age Colonists in Iberia

by BEATRICE BLANCE

Dr Beatrice Blance, a graduate of the University of Edinburgh, has for the last few years been engaged in research on prehistory in Spain, Portugal and Germany, as a Research Scholar of the Carnegie Trust for the Universities of Scotland, and as a Scholar of the Instituto de Alta Cultura in Lisbon. In this article she marshals the evidence for the establishment of East Mediterranean colonies in the Iberian Peninsula. Fig. 1 is a distribution map of some of the sites mentioned in the text. An appendix to this article lists the sites with bastions and with imported or imitation imported ware.

THE object of this paper is to show that certain Early Bronze Age sites in the Iberian Peninsula are actually colonies established by people coming from the Eastern Mediterranean.

The term 'colony' is used here in contrast to the term 'culture'. It is selected because, besides being the term used by Siret, who believed that Los Millares was a Phoenician colony, and the Leisners (*Factorei*), it is the term which best describes these sites. The following account will demonstrate that they were solitary, heavily-defended settlements situated in a culturally foreign environment. Their best parallels are to be found in the East Mediterranean area, where, from very early times politically independent city states which owed their existence to either a rich hinterland or to trade and commerce, are known. These sites in the Peninsula may, in fact, be regarded as primitive examples of the types of colonies established later by the Phoenicians and the Greeks.

In the following account the method of investigation is briefly described, but the chief advantages and disadvantages of each method cannot be discussed here. This side of the subject has been fully dealt with in a recent survey (Blance 1960). In this survey it was noticed that before the position of the colonies could be fully understood it was essential to take into consideration the Neolithic cultures present in the Peninsula. These can be briefly summarized as follows.

The earliest Neolithic groups in the Peninsula were the impressed-ware groups. Cardial impressed ware is found chiefly in North-East Spain and around the Tagus Estuary, while non-cardial impressed ware which is found in both these areas is also very common in Southern Spain. The cultural assemblage of these groups was less easy to ascertain, but it certainly included flint blades, trapeze-shaped arrowheads, and bone points and spatulae. The decoration on the pottery was nearly always confined to the upper part of the pot.

The Almeria culture was found principally in South-East Spain where some 150 graves are known, most of which seem to have been used for collective burial. A statistical analysis of the finds from these graves showed that they could be divided into two groups, which reflected a cultural rather than a chronological distinction. The 'indigenous' group was characterized by trapeze-shaped arrowheads, stone axes, bone points and probably also shell bracelets. The other group contained bifacially-worked flint arrowheads, flat idols, green-stone beads, and certain pottery types. It was thought that this group already in-

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licated early East Mediterranean influence in the Peninsula. There was evidence, too, that some of the Almeria graves were more likely to have been communal ossuaries than actual collective tombs, for example, Loma de la Torre 4, which was only 1.20 m. in diameter and 0.50 m. deep contained nine burials (G. and V. Leisner, 1943, 15).

This culture seems to have spread westwards, possibly due to the arrival of Los Millares people in the south-east, and to have played an important part in the formation of the



Fig. 1. Settlements and Cemeteries of the Early Bronze Age Colonists in Iberia.

Portuguese dolmen group, which is found chiefly in the Alentejo and South-West Spain. This group was characterized by trapeze-shaped and leaf-shaped arrowheads, stone axes, and decorated slate plaques, which are a later development from flat idols.

In North-East Spain there was an interesting local culture known chiefly from its 'Sepulturas de Fosa'. This culture, as the name suggests, was typified by single burial in earth graves; and the grave goods, which were very characteristic, consisted of stone axes with pointed butts, flint trapezes, bone points, and big olive-shaped callais beads. This group had clear Italian relationships and must be contemporary with the 'Neolitico Medio' of Arene Candide.

In Murcia and Alicante there was a special group of caves which appeared to have had some contact with all the Neolithic groups so far referred to (Monte de la Barsella, La Pastora, etc.).

Sites, which by general consent were dated to Early Bronze Age times, were next examined in order to find out how much of the earlier Neolithic influence could be traced in them.

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The two main sites were Los Millares and Vila Nova de S. Pedro.

Los Millares was a large, naturally-defended settlement site on a high promontory between two rivers in the present-day province of Almeria. Nearby was a cemetery of over seventy collective tombs. The settlement was defended by a stone-built wall which had bastions at regular intervals. There were complex devices for restricting and channeling the entrance to the settlement. It seems likely that there was also an inner defensive wall. A further defensive measure consisted of four small fortifications on the ridge of hills which bounded the site in the south-west. The graves were all collective tombs and most seem to have been corbelled. There were some, however, the construction of which suggested that they were probably to be regarded as 'round graves with passages'. During the 1955 excavations, it was noted that some graves had forecourts and often a cluster of baetyls or a scatter of pottery was found in them. A glance at the illustrations of the finds from these graves (G. and V. Leisner, 1943) showed that Neolithic features such as flat idols, trapezes, and green-stone beads, etc., were very rare at the site, and were found together in certain graves (e.g. grave 8). The majority of the finds from Los Millares could not be assigned to the Neolithic cultures nor could they be considered as belonging to the Beaker culture. The finds, in fact, consisted mainly of stone and bone containers, painted pottery, ivory and metal objects, baetyls or round idols, and symbol pottery. The distribution of these objects among the graves showed that there was a tendency for them to be found together, and very often in round graves with passages. A further study of the associated finds indicated that the phalanges, flint daggers, and deeply hollow-base arrowheads could also be included in this group. The graves containing objects typical of this group are: 7, 40, 9, 57, Loma de Huechar 2, 5, 8, 12, 15, 16, 23, 2, 25, 67, 10, 74, 41, 71, 21, 17, 32, 39 and 37. Thus at Los Millares it was possible to show that there was a certain assemblage of grave goods which cannot be related at all either to the earlier Neolithic cultures or to the later Bell Beaker culture.

A survey of the finds from the other sites in Almeria showed that, with the exception of Almizaraque, none of these sites had a typical Los Millares assemblage. Usually only isolated features typical of Los Millares were noted, and they occurred within a context more reminiscent of the Almeria culture (e.g. Loma del Belmonte 1; Llano de la Atalaya 3, 5 and 6, etc.). Almizaraque, a tell-like settlement site, situated in the Almanzora valley, yielded finds of a similar kind to those from Los Millares, and in addition a decorated stone sandal, many decorated long bones, and a pit which contained a bull burial (Cuadrado 1946). As at Los Millares there was also some slight evidence for contact with the Almeria culture (e.g. a flat idol and a stone bracelet, etc.).

In the neighbouring province of Granada, the position was even clearer, only a few objects typical of Los Millares were found in the graves there, and the characteristic grave goods were of Neolithic derivation.

The other main site, Vila Nova de S. Pedro, is situated near the village of the same name, about 9 km. north-west of Cartaxo, Ribatejo, in Portugal. This site is stoutly defended by three settlement walls, the two innermost of which are known to have bastions (Paço and Sangmeister 1956 and *Diário de Notícias*, Lisbon, 25 September, 1958). Recently three levels have been noted at the site. The earliest of these was found *under* the innermost defensive wall (here called the 'citadel') and consisted of a layer of earth which contained pottery decorated by stroke-burnishing (PLATES XXIV (c) and XXV). This pottery has sometimes been referred to in the publications as 'imported pottery'. The next level was represented by the citadel and its period of use. Both of these periods are called here for convenience V.N.S.P. I (Sangmeister (1956) refers to them as V.N.S.P. I and IIa). There are signs that the wall had been destroyed, hastily reconstructed, and destroyed again;

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above this level of destruction abundant sherds of beaker ware have been found. This level is called V.N.S.P. II (IIb of Sangmeister). As this distinction between V.N.S.P. I and II has only recently been noted, few of the finds can be attributed to either phase with any stratigraphical certainty. If, however, one dismisses the quantity of obviously beaker and later material there remains a group of finds which are similar to those known from Los Millares—stone and bone containers, round (or cylinder) idols, flint 'daggers', bifacially-worked arrowheads, and metal objects. To this list can be added the bone pins, which were replaced in beaker times by buttons, and the imported pottery and imitations of it, the stratigraphical position of which indicates an early date. Very little trace of Neolithic influence could be seen at Vila Nova de S. Pedro; of over six thousand arrowheads found for example, only three were trapezes and none was leaf-shaped; and only about a dozen slate plaques have been found. The evidence is, however, sufficient to suggest that although Vila Nova de S. Pedro owed nothing to the local Neolithic cultures, those cultures were still in existence in the area.

An examination of the finds from thirty-eight sites in the area around the Tagus mouth, which included settlements, rock-cut tombs, corbel-vaulted tombs, caves and dolmens, yielded most interesting and instructive results. With the exception of the corbel-vaulted tombs, none of the sites showed an assemblage of finds typical only of V.N.S.P. I, but rather there were indications of a mixture of V.N.S.P. I features and features derived from the Neolithic cultures. For example, grave 3 at Palmela, besides cylinder idols, typical V.N.S.P. I arrowheads and metal objects, also contained decorated slate plaques, sixteen trapezes and some leaf-shaped arrowheads; or the dolmen of Monte Abraão contained imitation imported pottery, cylinder idols, and bone containers, as well as slate plaques, leaf-shaped arrowheads and a flat bone idol.

The study of finds from the Tagus region made it possible to establish the typical assemblage for the V.N.S.P. I phase, which was imported pottery and imitations of it (PLATE XXIV), certain exotic pottery objects such as spit-supports, stone and bone containers, stone cylinder idols, all cult objects such as stone lunulae, pinecones, mattocks, horns, sandals, etc., bone pins, flint daggers, hollow-base arrowheads (especially mitre and Eiffel tower type), and metal objects such as axes, chisels, and one-sided knives (these knives are quite distinct from Beaker knives).

It is clear then that both Los Millares and Vila Nova de S. Pedro do not represent a developed stage of the local Neolithic cultures because they show so little contact with the Neolithic; nor do they indicate a widespread invasion of the Peninsula by new cultures, because the assemblage of finds from these two sites differs from the typical assemblage of finds from contemporary sites in their immediate neighbourhood. In fact, they stand out as isolated settlements against a different cultural environment. There are a few other sites which by their finds must be included in this category. These are Almizaraque, Alcalá, with its corbel-vaulted tombs (G. and V. Leisner 1943), and Mesas de Asta (Esteve Guerrero 1945 and 1950). The last-named is situated in the swampy zone near the mouth of the Guadalquivir, and north-west of the town of Jerez. Below the Roman and Iberic remains at this site the excavator found Beaker ware, and also sherds of pattern-burnished and monochrome red or grey ware.

The similarity of finds at these colonies has been emphasized. It must be added, however, that there are regional variations. For example, painted pottery is commonest in South-East Spain, while pattern-burnished ware is found in South-West Spain at Mesas de Asta and Acebuchal, and stroke-burnished ware in the Tagus area. Stone and bone containers in Portugal are either completely plain or decorated by a variety of motifs, those of South-East Spain are usually decorated only by incised cross-hatching; bone pin

types are simple in South-East Spain and more varied in Portugal; stone cult objects are more common in Portugal, but phalanges, sometimes decorated, are more common in South-East Spain. These differences are, however, regional, and do not hide the basic similarities between the sites.

It has long been recognized that sites such as Los Millares have parallels in the Eastern Mediterranean, though these have not always been easy to trace. The available evidence for East Mediterranean contact is briefly examined below in order to show that these colonies, whose culture is quite different from that of the Neolithic in Southern Iberia, are in fact closely related to East Mediterranean areas. It should be noted that when references are given below to East Mediterranean sites they are not meant to be exhaustive, but rather are intended to give an indication of the possible areas involved in the setting up of colonies in the Iberian Peninsula. These parallels will be considered in roughly the following order: architecture, pottery, stone, bone and metal objects.

Among the most striking of recent discoveries in the Peninsula is the use of complex defensive systems at the colonies. Bastions, complicated entrances, multiple enclosure walls and outlying defences are all known. The Los Millares entrance is so unusual that parallels cannot yet be pointed out in the East Mediterranean area.

Sangmeister has already drawn attention to the similarity between the Los Millares settlement wall and a similar wall with bastions at Chalandriani in Syros (Paço and Sangmeister 1956, 229; Tsountas, 1899, fig. 32) and with the exception of some later Egyptian examples (Somers Clarke 1916) this is, in fact, the only site in the East Mediterranean where semi-circular bastions are known because elsewhere, bastions, if they occur, are rectangular (Blance 1957; Schachermeyr 1955, fig. 57; Lamb 1936, 24, 44, 210).

The use of multiple enclosure walls is also known at Chalandriani, and even the narrow entrance in the citadel wall at Vila Nova de S. Pedro can be paralleled at this site.

A few stretches of herring-bone walling were noted in both the settlement wall and the graves at Los Millares, and this technique is familiar at East Mediterranean sites such as Troy, Thermi, Hagios Kosmas and Eutresis (Lamb 1936, 15; Mylonas 1934, fig. 2; Goldman 1931, 15, fig. 12).

Knowledge of the Neolithic cultures of the Peninsula shows that this type of fortification cannot have evolved independently in the Peninsula, and the evidence cited above suggests that it came from the East Mediterranean. It is probable too that the ultimate origin of the towers of the Sardinian Nuraghe and of some sites in Mallorca should be traced back to this same tradition which arrived in the Peninsula in Early Bronze Age times.

A long-standing enigma is that of the corbel-vaulted tomb. What is its origin? Such tombs are not known in the Eastern Mediterranean before Mycenaean times. Many unsuccessful attempts have been made to find exact parallels for the Iberian grave types, and here it is proposed only to show that certain features, common in the Iberian graves, can be paralleled in the East Mediterranean.

In the Peninsula, and particularly at Los Millares, the following features were noted in connection with 'corbel-vaulted' tombs: (a) probably not all were corbelled; (b) collective burial was practised; (c) offerings were made to the dead outside the graves, sometimes in a special niche; (d) the entrance to the graves was frequently constricted, usually by means of a port-hole slab; (e) the grave mound was often demarcated by stone slabs; (f) children were buried apart in niches (G. and V. Leisner 1943, 47, grave 20); (g) there was also a possibility that a grave of the Almeria culture had been used as a communal ossuary; (h) some rock-cut tombs were imitations of corbel-vaulted tombs.

Collective burial was known in the East from early times, when it was practised in

caves and rock shelters. Later these caves might be modified or enlarged for use as communal tombs, or certain houses might be set aside for use as communal ossuaries as, for example, in Gournia.

With regard to the various features noted in the Iberian graves the site of Haghios Kosmas is particularly interesting. There Mylonas found two types of graves, both of which had probably been used as ossuaries. One of these types had stone-built walls which inclined inwards giving them the appearance of primitive tholoi, and similar graves are known from Chalandriani in Syros (Mylonas 1934, 269; Tsountas 1899, figs. 5 and 10). The entrances to the graves at Haghios Kosmas were often very small and access must have been through the roof, and similar constriction of the entrance was noted in the Syros graves and at Krasi (Mylonas 1934, 268; Tsountas 1899, 83; and Marinatos 1929, 138). The area of the graves, too, at Haghios Kosmas, and also in Leukas, was often demarcated by a circle of stones (Mylonas 1934, 270, and Dörpfeld 1927, 207, 220). The custom of placing offerings for the dead outside the grave, often in a special niche, was a common practice at Haghios Kosmas, and was also noted in the Syros graves and in the Mesara tombs (Mylonas 1934, 271; Tsountas 1899, 80; Xanthoudides 1924, 51).

It is clear then that many individual features of the Iberian graves were also known in the East Mediterranean and it is not difficult to imagine that, owing to peculiar local conditions in South-East (?) Spain, the vaulted tomb, so typical of that area, arose.

A study of the pottery from the colonies is very interesting, and particularly the pottery from Mesas de Asta because, with the possible exception of El Acebuchal, it is the only site in the Peninsula which has yielded pattern-burnished ware.

In the Aegean, pattern-burnished ware is found in Neolithic and Early Bronze Age contexts. Mrs Ozanne (née Furness) considers that the pottery from Mesas de Asta is most closely paralleled in the material from Tigani in Samos, where pattern-burnished ware is characteristic of phase II. At Tigani, as at Mesas, both grey and brown pattern-burnished ware is found (the colours are most likely due to firing, rather than to any deliberate attempt to vary the colour); the pattern is frequently on the inside of the sherds, and the decorative motifs employed are also similar, with zones of cross-hatching occurring between completely burnished panels. Though this motif is fairly common in pattern-burnished ware, the cross-hatching at both Mesas and Tigani is noticeably elongated. At Tigani pattern-burnishing is found on deep open bowls. It is unfortunate that little information is available regarding the forms of this ware at Mesas, but open bowls must certainly have been represented there if the appearance of pattern-burnishing on the inside of the sherd is to be explained. A sherd from Orchomenos (Künze 1931, pl. ix, 3) has a plaited pattern-burnished design reminiscent of a sherd from Mesas.

Mrs Ozanne gives an excellent brief survey of the distribution of this type of pottery in the East Mediterranean (Furness 1956, 205-8). She was able to show that Tigani can be related to Besiktepe and Kumtepe Ia. Phase Ic at Kumtepe is contemporary with Troy I and thus Kumtepe Ia and related sites must be earlier than Troy I. From her survey it is obvious that pattern-burnished ware is confined to the Neolithic period and is probably earlier than Troy I.

Red monochrome and highly burnished grey wares were also found at Mesas de Asta; it is not easy to point to immediate parallels in the East. Besides being known at Sesklo, red ware, in the form of bowls with rounded profiles or incurved rims, was found at Tigani. The paste was not good and was covered by a red slip which tended to flake away. The slip had been applied to the outside surfaces only of certain of the vessels or to the inside of open bowls (Furness 1956, 187). The strawberry-red colour of the slip on the Mesas ware is more like the Tigani material than the orange-red slip of the Sesklo material.

It was also noticed in the grey wares at Mesas that the burnishing was restricted to the visible surfaces only. There is, however, no parallel in the shape of the vessels at Mesas and Tigani. Red or grey monochrome ware is found in Asia Minor at sites such as Bujuk Gullucek (Kosay and Akok 1948), Alisar and Alaca. Lunate or horseshoe lugs which are found at Mesas de Asta are known on dark burnished wares from several sites in Asia Minor, and also from Tigani, Troy I, Dimini, and Sesklo (Furness 1956, 185).

In Lipari, monochrome red ware is characteristic of the late Neolithic Diana culture. Although the shades of red are slightly different at Mesas and Diana, and Diana pottery is also characterized by trumpet lugs, which are never found in Mesas, some of the forms in both areas are similar. It is interesting to notice that according to Brea, bifacially-worked arrowheads (apparently hollow-base type) arrived with the Diana culture (Brea 1951, 94) and that the first signs of metallurgy were also encountered in a Diana culture level (Brea and Cavalier 1956, 35). At Diana, the red ware is not associated with grey ware or pattern-burnished ware.

White, or light on dark, painted pottery was confined to South-East Spain, and it was applied to various shapes of pottery. The decorated motif used was most frequently the herring-bone pattern, which was arranged either vertically or horizontally. G. and V. Leisner (1943, 589) compared this pottery with white-painted pottery of the E.M. III period in Crete. This technique, however, was not confined to Crete but was comparatively widespread. A recent study of the distribution of white-painted pottery undertaken by Mellaart (1954, 194 ff., 202 ff.) shows a concentration of this ware at a number of new sites in South-West Anatolia. He suggests that there are two phases in the use of this pottery, the first was contemporary with Troy I and the second was during the E.H. III–E.M. III period.

Stroke-burnished ware (and imitations of it) is found only in the Tagus area. Although it is known from about fourteen sites, Vila Nova de S. Pedro has undoubtedly yielded the most important finds. The finest sherds are of a very high quality, especially in comparison with the local Neolithic pottery, and the grooves are very regularly and finely executed (PLATE XXV). The typical form is a cylindrical vessel with slightly convex base and concave sides (compare with a white-painted vase from Belmonte I, G. and V. Leisner 1943, 127, I, 14), but the simple bowl shape is also found. Decoration usually consists of three or four rows of horizontal grooves below the rim, and above the base, and the space between may be left plain or it may be decorated by fine herring-bone patterns, which have been lightly burnished into the surface of the vessel. Little is known about the use of stroke-burnishing in the East Mediterranean, but Sangmeister has already drawn attention to the resemblance between this pottery and the Cycladic pyxides (Paço and Sangmeister 1956, 222, footnote; Tsountas 1898, pl. 9, 18 and 31). In the Cyclades, herring-bone decoration occurs mainly on squat cylindrical or on taller jars, but the pattern always appears to be incised into the pottery, and it is never confined to the surface as in the Portuguese material (our PLATE XXIV (c), and Zervos 1957, figs. 81 and 82). The Portuguese material is, in fact, of better quality and craftsmanship than the Cycladic!

The burnished red colour of the Portuguese sherds, together with their form and decoration, suggest that both they and the Cycladic pottery may have originated in attempts to copy decorated metal objects such as the jug from Tomb B at Alaca Hüyük (Arik, R.O., 1937, CLXXI, 242), although, of course, this is too late to be directly connected with the Cycladic pottery.

In view of the other Cycladic connections noticed at Vila Nova de S. Pedro it is perhaps not unreasonable to consider that this pottery has also some relationship with the Cycladic pottery.

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A mention must also be made of a pattern-burnished bowl from Aigina, which is remarkably like stroke-burnished vessels from Vila Nova de S. Pedro (Welter 1938, fig. 4, cf. Paço 1958, fig. 7).

Horned and plain cylindrical spit-supports were found at Vila Nova de S. Pedro (Jalhay and Paço 1945, xxi, fig. 7), but they are not known elsewhere in the Peninsula. Horned spit-supports, which are the more interesting of the two, were found at Asine, Troy, Thermi and in the Cyclades, while cylindrical ones with horizontal perforations are known from Olynthus, Eutresis, Rakhmani, Gozlu Kule, and Kusura (Frodin and Person, 1938, 232-3, No. 169-7; Sjövall, 1925, 185; Blegen *et al.*, vol. 1, fig. 369, 38-102; Lamb, 1936, fig. 44, 30, 6; Mylonas 1929, fig. 89a; Goldman, 1931, No. 266-4, 193; Wace and Thompson, 1912, 43, fig. 19; Goldman, 1956, 324, Nos. 16-21; Lamb, 1937, fig. 16, 4-6).

Stone containers which first appear in the Peninsula at this time are well known features of East Mediterranean cultures and the bone containers appear by their form and decoration to be closely related to them. There are a few instances of pottery imitations of stone vessels. Small pottery vessels with three or more mouths may be imitations of small stone vases of a type found in great numbers in the Cretan Tholoi (Xanthoudides 1924, pl. x). A few zoomorphic vases of stone or pottery are also known in the Peninsula.

While simple pin-types were found throughout Southern Spain, finds of vase and bird-headed pins were confined to the Tagus area. Sangmeister has already drawn attention to their similarity with the bird-headed pins from Syros (Paço and Sangmeister 1956, 225, fn. 13, and Tsountas 1899, pl. 10, 13), and there is also a vase-headed pin from this site *ibid.*, pl. 10, 14). Vase-headed pins and similar types are also known from early levels at Eutresis, Thermi, Troy, Alisar Hüyük and Gozlu Kule (Goldman 1931, fig. 284, Nos. 5, 7; Lamb 1936, 167; Blegen *et al.*, 1950, No. 37, 615; Van der Osten 1937, vol. I, fig. 196c, 2142; Goldman 1956, 313, fig. 438, Nos. 50, 51, 54).

The bone or ivory combs which have been found at several sites in the Peninsula are too simple to be able to show immediate East Mediterranean parallels.

The evidence suggests that the colonists introduced the knowledge of metal-working into the Peninsula and this included knowledge of casting in a closed mould (e.g. the Alcalá daggers). This is important because the succeeding Beaker phase used only hammered metal and heavy cast objects, such as axes, are not known in any Beaker assemblage.

The Alcalá dagger type with its bifacial mid-ribs is a general East Mediterranean type and similar dagger forms are known in the Cyclades, Crete, Lemnos, etc. (Xanthoudides 1924, pls. xxiv and xxix; Zervos 1956, figs. 290-292; Brea 1959, fig. 8). It is also suggested that some of the flint daggers and halberds so common in Southern Spain during this period may be imitations of some East Mediterranean dagger types such as those from Haghia Triada in Crete.

The axe type, typical of the colonies, is long with almost parallel sides and a lightly splayed cutting edge. Such axes are a common early type in many areas of the Eastern Mediterranean, and are often regarded as of Anatolian origin (Tsountas 1898, pl. 12, 7; Goldman 1931, fig. 286, 2; Brea 1959, fig. 9; Lamb 1936; Kosay 1944; Van der Osten 1937, 197, e 480, d 1877, c 2477; Kosay and Akok 1948, pl. xcix; Goldman 1956, 289; Lamb 1937, fig. 19, 1; Daux, 1959).

At Eutresis, in Boetia, a curved knife and a metal seal found there may be compared to similar objects from Vila Nova de S. Pedro and Monte do Cabeço dolmen (Paço and Sangmeister, 1955; the Leisners, 1959, pl. 16, 1, 1).

Mention must also be made of a painting, which could be interpreted as a ship with a high prow and stern and many oars, on an upright stone in the dolmen of Antelas, near Aviero (Albuquerque, Veiga Ferreira and Viana, 1957).

There can be no doubt that, despite regional differences, some of which may be due to the difference between grave and settlement finds, there is a remarkable uniformity between the main colonial sites, Los Millares, Almizaraque, Alcalá and Vila Nova de S. Pedro. Some of the differences may be due to the fact that each colony had a different source in the Eastern Mediterranean. Other differences may have developed as the colonies became cut off from their motherland and also, possibly, from one another. Thus the use of criss-cross decoration on the bone containers of South-East Spain may be an early feature, since this motif is known from early objects, such as the ivory sandal in Los Millares 7 and the ivory comb in Marcella. The variety of design on the bone containers in the West may, on the other hand, have been a later, local development. Differences, such as the presence or absence of spit-supports, vase-headed pins, or white-painted pottery, are more likely to be the result of different East Mediterranean sources. The influence of the Cyclades, the West Anatolian Islands and Anatolia is clearly suggested by the study of the finds. It would probably be wrong to regard any one of these areas as being responsible for the planting of all the colonies in the Iberian Peninsula, and it is likely that each of the main sites in the Peninsula was colonized from a different area in the Eastern Mediterranean.

Vila Nova de S. Pedro, which was shown to have several links with the Cyclades, must belong to a period when herring-bone design was still popular there and before the typical Cycladic spiral pattern had been developed. This implies a time before the E.C. II period which is usually dated about 2400 B.C. (Matz, 1950).

Mesas de Asta shows little contact with the other colonies and the find of pattern-burnished ware makes it probable that it is of an earlier period. If the parallel with Tigani in Samos, the dating of which depends on the date of Troy I (c. 2700 B.C.) is correct, the site must be dated about 2700–2600 B.C. This date would coincide approximately with the arrival of flat idols, etc., in Almeria.

The East Mediterranean parallels to Los Millares are less easy to trace to one particular area, but there are obvious contacts with the Cyclades and, of course, the earlier Almeria culture was characterized by Cycladic type idols. A sample of carbon, found *below the tumble* of the settlement wall at Los Millares, was dated by carbon 14 to 2345 ± 85 B.C. (Schwabedissen and Otto, 1958, 142), and this date agrees well with the one suggested above for Vila Nova de S. Pedro. Sangmeister (1956, 229) also obtained a similar date for the beginning of Los Millares and Vila Nova de S. Pedro.

It is interesting that the Iberian relationships are with the Cycladic and West Anatolian areas, where sailing would have developed of necessity, and the inhabitants would be capable of undertaking long sea voyages, such as the route to Spain. It is, at first sight, surprising that this phase is not so evident in Sicily, Lipari, or Malta. This may be due to the fact that the colonists preferred to sail along the continuous coastline of the South Mediterranean, or they may have feared the risk of piracy on the part of the islanders.

It is permissible to conclude that the evidence available indicates that during the Early Bronze Age in the Peninsula there appears a group of sites which cannot be directly related to the earlier Neolithic cultures. These sites do, however, show close contact with the Eastern Mediterranean, and particularly with the Cyclades and Western Anatolia. The earliest of these sites is probably Mesas de Asta (c. 2700–2600 B.C.) and it is possible that the foreign elements in the Almeria culture, which is earlier than Los Millares I, also arrived about this time. Vila Nova de S. Pedro and Los Millares were founded later and they can both be dated to before 2400 B.C. It is suggested that later, when contact with the homeland ceased, these colonies developed local characteristics. There was probably more contact between them and the Neolithic inhabitants, who had already been influenced by the colonists even to the extent of imitating among other things their pottery and their

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building achievements. The fusion of these two groups may well be behind the origin of the beaker culture, which in time overthrew the colonies, whether by destruction or domination, so that by Argaric times little evidence of this golden age in Iberian prehistory remains.

APPENDIX

The following sites in Southern Iberia have *bastions*: Los Millares, walls of settlement and of fort (Santa Fe de Mondújar, Almería, Spain); Campos, a house (Cuevas de Vera, Almería, Spain); Vila Nova de S. Pedro, in at least two of the settlement walls (Cartaxo, Ribatejo, Portugal); and (?) Zambujal, walls of settlement (near Torres Vedras, Estremadura, Portugal).

The following sites in Portugal have yielded *imported pottery* either true or imitation: Castro de Chibannes, Palmela; Licea, Barcarena; Pedra de Ouro, Alenquer; S. Mamede, Obidos; Quinta do Anjo, Palmela; Alapraia, S. João de Estoril; Carenque, Belas; Monge, Serra de Sintra; Agualva, Caçem; Monte Abraão, Belas; Samarra, Acafora, Sintra; Arruda II, Freiria, Torres Vedras; Poço Velho, Cascais; Cova da Moura, Torres Vedras; Vila Nova de S. Pedro, Cartaxo, Ribatejo; S. Pedro de Estoril, Estoril; Parcede, Estoril; Gruta de Carvalhal, Turquel, Obidos; Cartaxo, Vila de S. Pedro.

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The First Farmers in Turkmenia

by V. M. MASSON

It is over half a century since Pumpelly made his archaeological reconnaissance in Turkmenia and dug at Anau. Since 1947 reconnaissance and excavation of Anau and comparable sites in southern Turkmenia have been conducted by the Joint South Turkmenian Archaeological Expedition, directed by Professor V. M. Masson and the Institute of History and Archaeology of the Turkmenian Academy of Sciences. Here Professor Masson summarizes the results of more than a decade of this important work.

THE early farming culture of Southern Turkmenistan, widely known since the work of R. Pumpelly at Anau, has been systematically studied during the last decade by Soviet archaeologists. Excavation of the major settlement of this culture, Namazga Tepe, has made it possible to propose a new stratigraphy (Namazga periods I–VI).¹ The Namazga III period fills the gap between Anau II and Anau III, and has its closest analogies on Iranian sites such as Sialk III, 4–7, and Hissar IB to IIA; it is now possible to revise the synchronization of sites in Turkmenia and Iran proposed by McCown.² (See table on p. 212).

In continuing our work we have concentrated attention on the excavation of early settlements of the Namazga II–III periods. The task has been carried out by a special group of the Joint South Turkmenian Archaeological Expedition under the direction of the writer of this article, together with I. N. Khlopin and V. I. Sarianidi as assistant directors throughout. At the time of writing the results of our work during the 1955–8 seasons have been fully published³ and the report of the excavations of 1959–60 is in preparation. The present article is intended to be merely a short survey of the material and of the problems arising in the study of it. (For sketch-map of sites, see FIG. 1).

When systematic excavation of early farming sites began in 1955, its main purpose, besides establishing the stratigraphy, was an extensive excavation of the upper levels of sites of various periods. Establishing the plan of the settlements undoubtedly helped towards the study of the economic and social life of the inhabitants, and the choice of sites of various periods gave us the hope of establishing a complete historical sequence.

Of the known sites the earliest settlement of settled farmers in South Turkmenistan is Djeitun (or Jeitun), 30 km. N.N.E. of Ashkhabad. Its area is small—about 4000 sq. m. In 1958–9 about three-fifths of this most interesting site was uncovered. The basic type

¹ *Trudy Yushno-Turkmenistanskoy arkheologicheskoy kompleksnoy Ekspeditsii*, VII (1956); V. M. Masson, 'Djeitun and Kara Tepe', *Sovetskaya Arkheologiya*, 1957, No. 1, 143–60; M. E. Masson and V. M. Masson, 'Archaeological Cultures of Central Asia of the Aeneolithic and Bronze Ages', *Cahiers d'histoire mondiale*, 1959, 1–40.

² *The Comparative Stratigraphy of Early Iran*, Chicago, 1942.

³ V. M. Masson, *Vestnik Drevnei Istorii*, 1957, No. 1, 34–47; *Trudy Y-T.A.K.E.*, 1960.

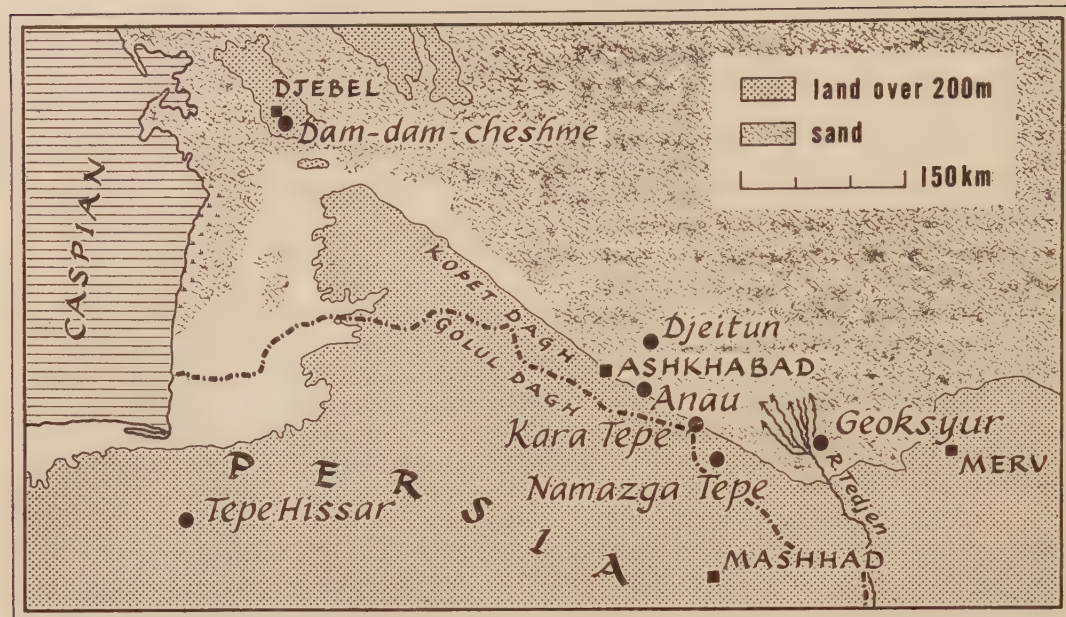


Fig. 1. Sketch-map showing sites discussed in this article, and the Turkmenian-Iranian boundary.

of dwelling was a small one-roomed house of standard plan, varying in area from 20 to 35 sq. m., with the exception of three small houses of 13, 16 and 18 sq. m. Inside, the floor was as a rule covered with a thick lime-plaster, sometimes retaining traces of reddish-brown paint. Along one of the walls was a large square hearth, and on the wall opposite a small projection, usually painted red-brown or black, with a small niche low down near the floor. One of the corners of the house near the hearth was partitioned off by a small wall to form a sort of storage bin. In the excavated part of the settlement of Djeitun nineteen houses of a similar type existed at the same period, each with its small yards and farm and storage buildings adjoining.

The finds from the excavation give us a sufficiently clear picture of the culture and economy of Djeitun. Of importance here is the microlithic flint industry with trapezes, borers, various types of micro-scrapers, concave-trimmed and notched blades and trimmed blades, etc. Sickie-handles were widespread: some 300 were found. Bone implements included borers, needles and tools for scraping skins, made of animals' shoulder blades.

The pottery was hand-made, and the clay was tempered with large pieces of chopped straw. The main forms were cylindro-conical storage vessels, cups and square 'salad bowls'. The storage vessels were decorated with reddish-brown paint on a cream background; the decoration was extremely simple, consisting mainly of vertical rows of wavy or 'bracket-like' lines (FIG. 2).

The inhabitants of Djeitun were agriculturists; the impressions of grains of barley and wheat have been discovered on the clay blocks of which their houses were built. Most of their meat however was still provided by the hunting of bezoar, goat, *ovis orientalis* and *gazella subgutturosa*. Only a small proportion is represented by goats and sheep with signs of domestication.

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Fig. 2. Type objects of the Djeitun Culture.

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Material analogous to that at Djeitun is known from a number of other sites, so that we may speak of the existence in South Turkmenistan of a distinct Djeitun culture preceding in time Anau I–Namazga I. In a number of points, for example, the form of the storage vessels with the body tapering towards the base, a genetic link may be traced between the Djeitun culture and Anau I–Namazga I. The Djeitun culture can be tentatively placed in the 5th millennium, though it may in part have to be dated even earlier.

The discovery of the Djeitun culture is of considerable interest from the point of view of the problem of the origin of the early farming cultures of South Turkmenistan and Iran. In this respect two lines of investigation can be noted; first the links of the Djeitun culture with the Iranian and Turkmenian Mesolithic which has become known in recent years, and secondly the links of Djeitun with a number of early farming cultures in the Near East.

Excavations in the Caspian area by A. P. Okladnikov⁴ and Carleton Coon⁵ have revealed a picture of Mesolithic tribes of fishermen and hunters of *gazella subgutturosa*. As we have seen, hunting retains its importance at Djeitun, and the microlithic flint industry of the site is apparent to the flint industry found in the upper levels of the Djebel cave. The conical terracotta objects found at Djeitun may most probably be derived from the cones of unbaked clay described by Coon from levels 10 and 11 at Ghar-i-Kamarband.⁶ One must bear in mind that in the Mesolithic of the Caspian area, because of the local conditions, the gathering of wild cereals was not well developed. It is of course possible that at the time of the occupation of the Djebel and Ghar-i-Kamarband sites the valleys in the Turkmeno-Khorassan mountains were inhabited by Mesolithic tribes engaged in gathering wild cereals and that the culture of these people might be one of the ancestors of Djeitun.

The Djeitun culture has clear links with the early farming settlements of the Near East. In one of the houses at Djeitun there was a storage pit whose walls were plastered with fired clay. Similar pits seemed to have occurred at Jarmo and Jericho and a number of other Near Eastern sites. Then identical conical terracotta objects have been found at both Djeitun and Jarmo.⁷ And judging by Dr Kathleen Kenyon's description the buildings of the pre-Pottery Neolithic B of Jericho have a number of features in common with those of Djeitun. The basic type of dwelling in Jericho was one with large rooms 6.5 m. × 4 m. or 7 m. × 3 m. in size, the floor covered with plaster often painted red or cream (cf. Djeitun). Around such central large rooms were huddled small buildings for storage purposes.⁸ However, despite all these links, the Djeitun culture preserves an individuality against this general background of peasant-village communities.

In discussing origins the most essential fact is the shape of the bone sickle handle found on one of the Djeitun sites; it is characteristic of the Natufian. The existence of this sickle handle among the early farming communities of Turkmenia, as also in Iran,⁹ may perhaps be evidence of links between the Mesolithic of those areas and the Natufian of Palestine, or possibly of a movement eastwards of people from Syria and Palestine. The fact that there are certain common elements in the culture of the early farming communities of the Near East may mean no more than that there was a community of culture among the preceding Mesolithic peoples in that area who were gathering wild cereals. This complex problem obviously needs more investigation.

⁴ Jebel, Dam-dam-cheshme, etc., in *Trudy Y-T.A.K.E.*, 1956.

⁵ *Cave Explorations in Iran*, 1949 (Philadelphia, 1951) and *Seven Caves*, 1957.

⁶ *Cave Explorations in Iran*, 1949, 75.

⁷ R. Braidwood, *The Near East and the Foundations for Civilisation* (Oregon, 1952).

⁸ K. Kenyon, *Digging up Jericho* (London, 1957), 53–5.

⁹ Sialk I, vide Ghirshman, *Fouilles de Sialk*, II, 1939.

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What can now be said about the origins of the Djeitun culture? It must surely be primarily connected with the Mesolithic tribes of Iran and Southern Turkmenistan. Perhaps further archaeological work in those areas will lead to the discovery of an intermediate stage in the development of the early farming cultures, and one that may be parallel to the pre-pottery Neolithic of Jericho and Jarmo.

In the following period there developed from the Djeitun culture another culture very similar to Anau I, Namazga I and Sialk.¹⁰ To this horizon (i.e. Anau I–Namazga I) belongs the site whose plan has been most fully studied, namely, Dashlydji Tepe in the Geoksyur oasis. This oasis, 20 km. east of the River Tedjen, was once watered by the ancient delta of the Tedjen; it is now a desert-steppe and contains a group of ancient settlements. Nine of these have been located and are referred to as Geoksyur 1–10. Dashlydji Tepe is Geoksyur 8 and is in the north of this group of sites. It is a small hill about 1200 sq. m. in area containing the remains of three building levels which have been fully excavated. The most characteristic are the structures of the second building level: all are built of unbaked brick with dimensions of 46–48 × 24 × 10 m. and 36–38 × 24 × 10 m. There is a group of four buildings consisting of a living-room, roughly square in plan with a small hearth, and various storage buildings of irregular plan. A settlement of the Dashlydji Tepe type has clear links with the Djeitun settlements.

Finds during the excavation of Dashlydji Tepe included copper and bone points, terracotta figurines of standing women of a marked steatopygous appearance, many animal figurines of unbaked clay, and pottery of various types. The pottery is the same from all three building levels and belongs to the well-known Anau IB–Namazga I type. Among the decorated wares the commonest decoration is in a dark brown paint on a greenish-white background. A red slip was sometimes used as a background but only rarely, and this distinguishes the pottery of Dashlydji Tepe from the pottery of sites further west such as Anau, Kara Tepe and Namazga. At Namazga, and among the Namazga I pottery from Kara Tepe, decoration on a red background is very common. The decorative motifs include rows of outline triangles, ribbon patterns on the rim, triangular chevrons dependent from the rim, and trees. For a variety of reasons this pottery can be dated to the later stages of Anau IB–Namazga I. It is also found on two other sites in the Geoksyur oasis, namely, in the lower levels of Geoksyur 1 and in Geoksyur 2. The Namazga I levels in the Geoksyur oasis are not thick—2 to 2.5 m. (two or three building complexes), especially when compared with the settlements of the western group (Kara Tepe: 7 m. and Namazga Tepe: 6 m.). The Geoksyur oasis was occupied by early farming tribes only in late Namazga I as a result of an expansion from the area to the west (Anau and Namazga Tepe). The culture of the Geoksyur oasis at this period is very similar to that of the 'metropolis'.

In the Anau II–Namazga II period the differences in culture become more noticeable, and we are now justified in speaking of two variants of the early farming culture of Southern Turkmenistan—a western and an eastern. The eastern group has been best studied in the excavation of the sites in the Geoksyur group. Without going into the more detailed stratigraphy established by the excavations at Kara Tepe and Geoksyur it must be made clear that at present we can distinguish quite definitely two periods, namely, early Namazga II and late Namazga II.

In the early Namazga II period pottery with polychrome decoration of the type known from Anau II is widespread on the western sites. The excavations at Kara Tepe have established that the decoration included, besides the geometrical motifs, geometrized human figures and schematic drawings of goats. In the Geoksyur oasis the settlements

¹⁰ In Iran sites of a corresponding type are still unknown.

of Yalangach Tepe (Geoksyur 3), Mullali Tepe (Geoksyur 4), and Geoksyur 2 (the upper levels) belong to this period. The planning of the houses at Yalangach Tepe and Mullali Tepe is very remarkable. Around the edge of each of them ran a circuit wall between 50 cm. and 1 m. thick. Included in its perimeter at irregular intervals were oval rooms of between 3.5 m. and 4.5 m. in diameter. Judging by the presence of hearths in these rooms the rooms were lived in. Within the circuit of the walls the houses consisted of a large number of living and storage rooms. The living-rooms were roughly square in plan with a rectangular hearth in one of the corners, and closely recall analogous structures at Dashlydji Tepe. However, they were not placed apart but close together; at Yalangach Tepe, for example, they are in groups with walls abutting. Here we can see, as it were, a stage in the growth of the large many-roomed house characteristic of the following period. But here we have not as yet a many-roomed house built on a unified plan, but still separate single-roomed houses built close together.

The finds from Yalangach Tepe provide especially important evidence for the culture of the period. They included flat copper spearheads, part of a copper axe, flint arrowheads with serrated edges, bone awls, a bone pottery burnisher, animal figurines of bulls and goats in unbaked clay, and female figurines—the most typical being the large figurine of a seated woman with solar symbols painted on her thighs (FIG. 3). This pose is repeated unchanged in a figurine of a later period (late Namazga II and Namazga III), but the figurines themselves become smaller and more elegant.

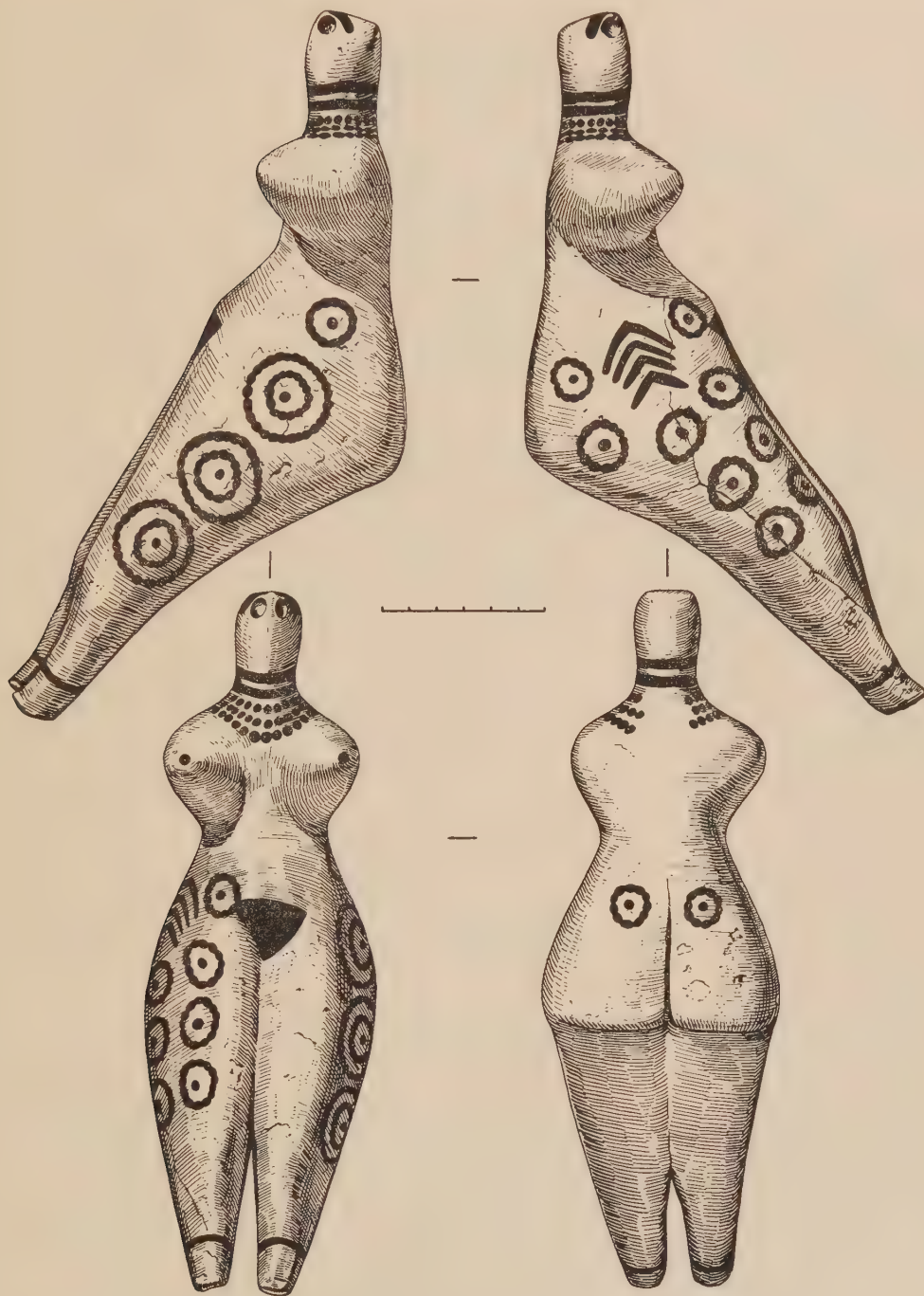
The decorated pottery found at Yalangach Tepe may be divided into three groups. The first and largest group comprises pottery decorated with four parallel lines along the rim and large pots with triangular chevrons pendant from the rim. This Yalangach Tepe ware is characteristic of the eastern group of sites and has a clear genetic connection with the pottery from Dashlydji Tepe. The second group, on the other hand, consists of pottery characteristic of the western sites—magnificent vessels with Anau II polychrome decoration. The quantity of this is not large and is perhaps evidence that the Geoksyur oasis continued to receive new influxes of population from the west. Finally, the third group consists of pottery decorated with geometrical figures (oblique hatched lozenges) clearly analogous to Sialk II pottery; the quantity of pottery in this group is also small.

The late Namazga II period is characterized by the appearance of large many-roomed houses built on a unified plan. Parts of such houses were excavated at Kara Tepe.¹¹ Here at this period polychrome decoration on pottery almost disappears and the predominant type of pottery has dark brown decoration on a yellow or pinkish background. The geometrical decoration entirely continues the traditions of the polychrome pottery. Widespread are schematic linear drawings of goats, identical with the drawings on pottery from the Elamite settlements of Juivi and Band-i-bal investigated by L. le Breton and which antedate Susa I.¹²

The best studied site of this period in the eastern group is Geoksyur 1—the largest settlement (about 11 hectares) and clearly the ancient centre of the whole oasis. Here several many-roomed houses separated by narrow alleys have been partially excavated. On the edge of the settlement was a special cemetery where the dead were placed in chambers built of unbaked brick. The chambers were oval or rectangular with a false vault of unbaked brick, containing as a rule several burials of different periods. Such structures are not known earlier in Southern Turkmenistan, although burials of the normal type in pits sometimes lined with unbaked brick are known in large numbers, especially at Kara

¹¹ E.g., V. M. Masson, *Sovetskaya Arkheologiya*, 1957, No. 1, 147, fig. 4.

¹² L. le Breton, *Mémoires de la Mission Archéologique en Iran*, 1947, fig. 29, 1-7, fig. 40, 1-3, 9, 10.



*Fig. 3. Female figurine from Yalangach Tepe: Early Namazga II.
(Scale in cms.)*

ANTIQUITY

Tepe. The burial chambers of Geoksyur may be compared with the cemetery of Musyan, and it is not impossible that their appearance in Southern Turkmenistan reflects ancient cultural links reaching far to the south-west. The pottery of the late Namazga II period on the eastern sites is very distinctive. The Geoksyur ware with its bright bichrome (black and red) decoration has a rich repertoire of geometrical decoration, including crosses, half-crosses, serrated lines, etc. Even the animal figures take on a geometrical outline. The Geoksyur ware has little connection with the preceding Yalangach ware. On two sites of the Geoksyur group a layer was discovered separating the levels containing Yalangach and Geoksyur ware, and in this intermediate layer almost all the pottery was undecorated. Red burnished, red slipped and also grey wares were common. The decoration of the Geoksyur style can often be explained by assuming that its motifs were due to the influence of decoration used in other applied arts such as weaving and carpet-making, appliqué-work, etc. Indeed this may help to explain the whole origin of the Geoksyur style pottery. It is also probable that new archaeological discoveries in the Meshed-Kuchan area will shed new light on its origin.

The most important excavations were those carried out at the site of Kara Tepe near Artyk. This site has many strata and has produced much material of the Namazga I and Namazga II periods, but its most important role is in the study of the Namazga III period. The occupation of the site ceased during this period; this final settlement has been extensively examined. From this level we obtained a radio-carbon date of 2750 ± 220 B.C. Stratigraphy showed that at about this time Geoksyur was deserted; we can then date the beginning of the use of Geoksyur ware to the beginning of the Third millennium B.C. (On other sites in the eastern group Geoksyur ware continued in use into the Namazga III period.)

The eastern part of Kara Tepe was excavated and in the centre of the settlement was found a large square from which ran narrow streets separating the large many-roomed houses consisting of fifteen to twenty rooms. Situated among the rooms were small inner courtyards 40 to 50 sq. m. in area. Alongside one of these houses we uncovered a large yard about 250 sq. m. in area. The rooms were of two types (a) large rooms from 20 to 25 sq. m. with a small hearth in the centre, and (b) small and usually narrow rooms 4.5×1.8 m. The large rooms were almost certainly living-rooms; the hearths consisted of a vessel with an open bottom let into the floor and, like the present-day Uzbek *sandal*, served to heat the rooms. The smaller rooms were storage rooms or bins; the floor and walls of some of them were faced with fragments of pottery. The kitchen, in those cases where its identity could be established, was in an inner court and was common to a whole complex of rooms.

Among the many finds from Kara Tepe (mainly consisting of copper and bone points, querns, mortars, vessels of white and pink stone simulating marble, flat seals of stone and clay marked with cross-impressions, spindle whorls, etc.) there was an especially rich collection of miniature sculpture and many types of decorated pottery including about 150 whole pots. Among the figurines of stone and baked clay special mention must be made of the massive figure of a bull of white marble-like stone and the terracotta figurine of a cow painted with dark brown spots clearly representing the brindling of live cows. The wheels of model carts were also found. The pottery, all without exception hand-made, is represented by wares with dark brown decoration on a greenish-white background, and with the same colour decoration on a red background, by grey ware, usually burnished, and coarse kitchen ware with an admixture of grit (large cooking pots, vessels for storing food, etc.). The pottery with a red background and the grey ware were comparatively rare. The most popular form among the decorated pottery was a deep cup. The rich

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geometrical ornament shows traces of the influence of the Geoksyur style, but the decorative elements are now smaller (PLATE XXVII).

For the first time in Southern Turkmenistan animals other than goats are used as decorative motifs on pottery. These are mainly birds, usually in combination with solar circles, and spotted animals clearly recognizable as the snow-leopard of Sialk and Hissar. Much more rare are elongated figures of cows and eagles with outstretched wings, closely recalling the figures on the pottery of Susa and Tall-i-Bakun. Human figures are known but these are comparatively rare.

It is especially noticeable that only wild animals were portrayed at a time when hunting was of minimal importance in the life of the inhabitants of Kara Tepe (the bones of wild animals make up about 5 per cent of the total). It may well be that these animal representations are connected with earlier representations and indeed may, it seems to me, reflect a survival of totemism. Thus, in Southern Turkmenistan the most popular totem was that of a mountain goat, depicted for the first time on late Namazga I pottery and surviving until Namazga VI.¹³

The considerable new material produced by the excavations of the last few years raises a number of interesting questions connected with the ancient history both of Southern Turkmenistan and of neighbouring areas. In this present article we propose to dwell on two only of these issues. We have shown how the new discoveries show a line of evolution in the planning of settlements from the single-roomed houses of Djeitun to the many-roomed complexes of Geoksyur and Kara Tepe. We must make this reservation, namely, that not all details of this process have been studied sufficiently fully, but the general trend seems clear and well established. The houses of Djeitun were, or so it seems to me, the homes of single families which together formed a tribal settlement. The houses of Geoksyur and Kara Tepe with their communal kitchen and five to seven living-rooms were the dwelling places of a group of related families recalling in some measure the large house of the Iroquois or the buildings of the Pueblo Indians. Settlements like Kara Tepe and Geoksyur consisted of the houses of such 'family groups' into which the organization of a tribal settlement had disintegrated. This problem, naturally, demands further study and in particular the discovery of new evidence. It is to be noted that the houses of Tall-i-Bakun and Sialk III, that is to say, settlements of a fairly developed period of the early farming cultures of Iran, find close analogies in the houses of Geoksyur and Kara Tepe. Especially interesting in this connection is Dr Kenyon's statement that in the lowest levels at Jericho—the earliest known settled farming community in the Near East—oval houses have been excavated which have no internal partitions, that is, they are really single-roomed houses.¹⁴ Mention has already been made of the similarity between the houses of the pre-Pottery Neolithic period and Djeitun. We must consider the study of ancient planning to be one of the most important methods of investigating the early farming cultures of the Near East.

The second question that demands discussion is the problem of the connections of the early farming settlements of Southern Turkmenistan. Some of the parallels have already been mentioned: they are clearest in such periods as Hissar IB–IIA and Sialk III 4–7 on one side and Namazga III (in its early stages studied at Kara Tepe) on the other. The appearance among the motifs on the decorated pottery of Kara Tepe of animals such as leopards, birds and eagles cannot be explained except by the influence of the culture of Central Iran. There are grounds for supposing that this influence is connected with the

¹³ V. M. Masson, 'The Ancient Farming Culture of Margiana', *Materialy i issledovaniya po archeologii S.S.S.R.*, 1959, No. 73, pl. VIII.

¹⁴ K. Kenyon, *ANTIQUITY*, 1959, 5–9.

ANTIQUITY

Anau	Namazga	Sialk	Hissar	Shah Tepe
	VI	V		
III	V		III C	II A
			III B	
	IV		III A	II B
		IV	II B	
		III 7	II A	III
	III	III 6		
		III 5	I C	
		III 4		
		III 3	I B	
II	II	III 2		
		III 1	II A	
		II		
I B	I			
		I		
I A				
Djeitun				

Table to show comparative stratigraphy of Turkmenian and Iranian sites.

movement of certain people into Southern Turkmenistan. For example, in Namazga III the old tradition of the burial rites of Kara Tepe is affected by the introduction of different methods of laying out the corpse and a different orientation. Ginsburg and Trophimova find in these same burials the presence of physical types found in Sialk.¹⁵ The newcomers were, however, quickly assimilated by the local population, for we see the old traditions preserved, especially in the shapes of the pottery; the pottery continued to be hand-made at a time when the potter's wheel was already in use at Hissar and Sialk.¹⁶

The other line of connection is with Southern Afghanistan and Pakistan. The culture

¹⁵ *Trudy Y-T.A.K.E.*, 1960.

¹⁶ E. Schmidt, *Excavations at Tepe Hissar* (Philadelphia, 1937); R. Ghirshman, *op. cit.*

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of Quetta in Northern Pakistan¹⁷ has produced a striking number of analogies with the South Turkmenian sites, namely, seals, figures and a large number of motifs on decorated pottery. In addition it is precisely these motifs which are not characteristic of the Baluchistan group of cultures as a whole and are found primarily on the Quetta pottery. This group of Quetta pottery may therefore be connected with the influence of the Geoksyur style in the form in which it is reflected in Namazga III and IV pottery. On the basis of radio-carbon dating the Quetta pottery belongs to the second half of the third and the beginning of the second millennium B.C. Perhaps its link with South Turkmenian sites can be explained by an infiltration into the Quetta area of some tribal groups from the north-west.

Even before this, the material from Rana Ghundai showed links between the sites of Northern Baluchistan and the Hissar culture.¹⁸ New excavations at Mundigak have provided remarkable confirmation of this.¹⁹ Evidently the third millennium was a time of considerable movement among the early farming tribes grouped around the Iranian plateau. In this connection the study of the Kuchan–Meshed–Gerat area seems highly desirable, for it is surely here that we may expect the mixing of cultures from Hissar and Geoksyur which exercised an influence on Quetta and Mundigak.

¹⁷ S. Piggott, *Ancient India*, 1947, 131 ff. and *Prehistoric India* (1950), 73; W. A. Fairservis, 'Excavations in the Quetta Valley', *Anth. Papers of the American Museum of Natural History*, 1956, 2.

¹⁸ See S. Piggott, D. McCown, *op. cit.*, 58–9, and V. G. Childe.

¹⁹ J. M. Casal, *Quatre campagnes de fouilles à Mundigak*, 1951; *Arts Asiatiques*, 1954, 163–178.

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Worsaae and British Antiquities

by JUDITH WILKINS

Miss J. M. Wilkins is a Research Student at Girton College, Cambridge, working on the historical development of ideas in prehistoric archaeology. She here discusses the influence of J. J. A. Worsaae (1821-1885) on British prehistory in the second half of the 19th century.

J. A. WORSAAE'S recorded words during his travels in the British Isles in 1846-7 indicate that he neither underestimated the significance of his visit, nor lacked confidence in the tenability of the ideas he expressed. His previous journeys in Norway, Sweden and Germany were a useful preparatory experience. Notwithstanding his youth and his high spirits, Worsaae must have appreciated his influential position as the exponent in Britain of 'an entirely new enquiry into the history of the earliest state of the European nations, by means of the antiquities alone'. The discovery of 'a stone period, in the history of Europe' was, he declared, the first achievement and the vindication of the method.¹ Worsaae's presence in Britain implies a certain if admittedly confined interest in antiquities. This is confirmed by his discovery that William Thoms had translated *Danmarks Oldtid* from the German edition, as a source of comparative material, without the author's knowledge and before his arrival in Britain.²

During the nine months he spent travelling in England, Scotland and Ireland apparently Worsaae gave only two formal lectures, to the Royal Irish Academy in Dublin on 30 November and 14 December, 1846; though he commented also on several papers—such as one on a Viking hoard from Cuerdale—given at the Society of Antiquaries of London during this period. The media for the circulation and propagation of his ideas were to a great extent his personal contacts, that were in many cases initiated during his first visit. Worsaae's letters to his mother³ describe his enthusiasm for making new acquaintances, and he told her of introductions to a variety of British antiquaries at this time, including the irascible Charles Roach Smith, Charles Newton the classical archaeologist, Edward Hawkins, Keeper of Antiquities in the British Museum since 1826, and the youthful Augustus Franks, who was to join the staff in 1851 and to become 'one of the most remarkable men that the Trustees have ever had in their service'.⁴

Many of these early introductions resulted in a persistent and ever-expanding correspondence between Britain and Copenhagen, that maintained the contact evident from Worsaae's articles in British periodicals and the translations of certain of his later books.⁵

¹ *Proceedings of the Royal Irish Academy*, III, 1845-7, 328-9.

² First published in Copenhagen in 1843. Thoms's translation was published in 1849 as *The Primeval Antiquities of Denmark*.

³ Published in *En Oldgrandsketers Erindringer*, ed. V. Hermansen, 1934. I am grateful to Mr T. G. Bibby of the Forhistorisk Museum, Aarhus, for drawing my attention to this publication.

⁴ T. D. Kendrick, 'The British Museum and British Antiquities,' *Museums Journal*, 51, 1951-5, 144; and in *ANTIQUITY*, 1954, 132.

⁵ *An account of the Danes and Norwegians in England, Scotland and Ireland*. 1852.

The Industrial Arts of Denmark from the Earliest Times to the Danish Conquest of England. 1882.
The Prehistory of the North. 1886.



(a)



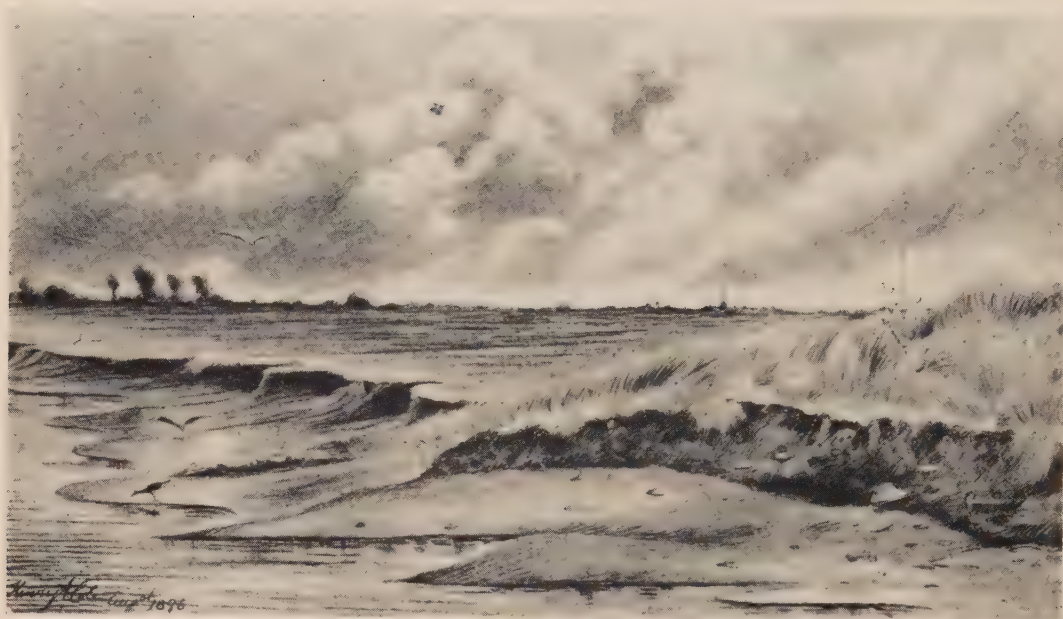
(b)

SIR JOHN MARSHALL AND GANDHĀRA ART

(a) *Fragment of clay figure from one of the niches in the peribolos wall of the Kanishka Sanctuary on the acropolis at Surkh Kotal.* (b) *Remains of one of the colossal Buddhist figures on the 'platform in the plain' at Surkh Kotal.*

PLATE XXII

(a)



(b)



PREHISTORIC SALT-BOILING

(a) Red Hill on the east coast of the Isle of Mersea in Essex; (b) Red Hill excavation of the year 1888
(both after Cole 1906.)

PLATE XXIII



(a)

(a) *Pillar- and dish-shaped salt cakes at Bilma in Central Sahara (after Foureau 1902).*



(b)

PREHISTORIC SALT-BOILING

(b) *Bowls used for the production of moulded salt set in a salt-boiling oven in Manga (Sudan) (after Foureau 1902).*

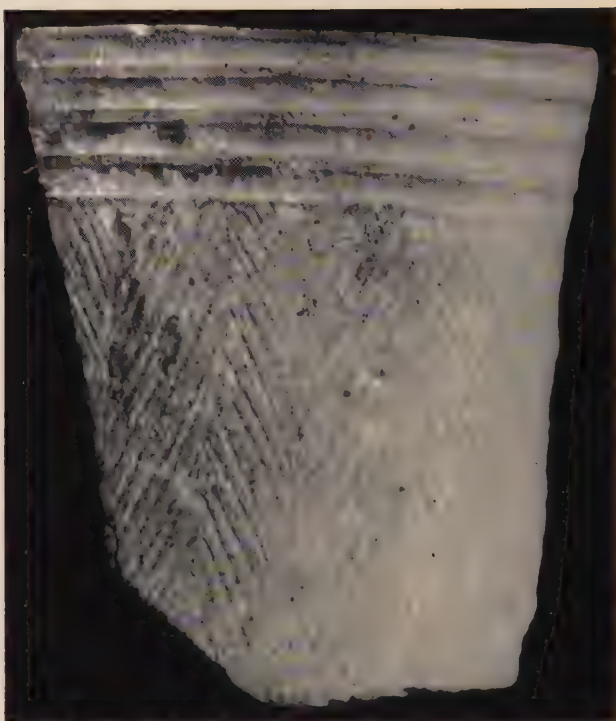
(a)



(a) Sherd of coarser pottery from Vila Nova de S. Pedro, probably an imitation of the imported pottery.

(b) Pyxides from the Cyclades.

(c) Fragment of stroke-burnished 'imported pottery'.



(c)

(b)





EARLY BRONZE AGE COLONISTS IN IBERIA

Magnification of the stroke burnished pottery in PLATE XXIV(c) to show the fineness of the technique. This is 'imported pottery' of the type found under the citadel wall, at Vila Nova de S. Pedro.



THE FIRST FARMERS IN TURKMENIA
Kara Tepe: Namazga III decorated pottery.



THE FIRST FARMERS IN TURKMENIA
Kara Tepe: Namazga III decorated pottery.

PLATE XXVIII



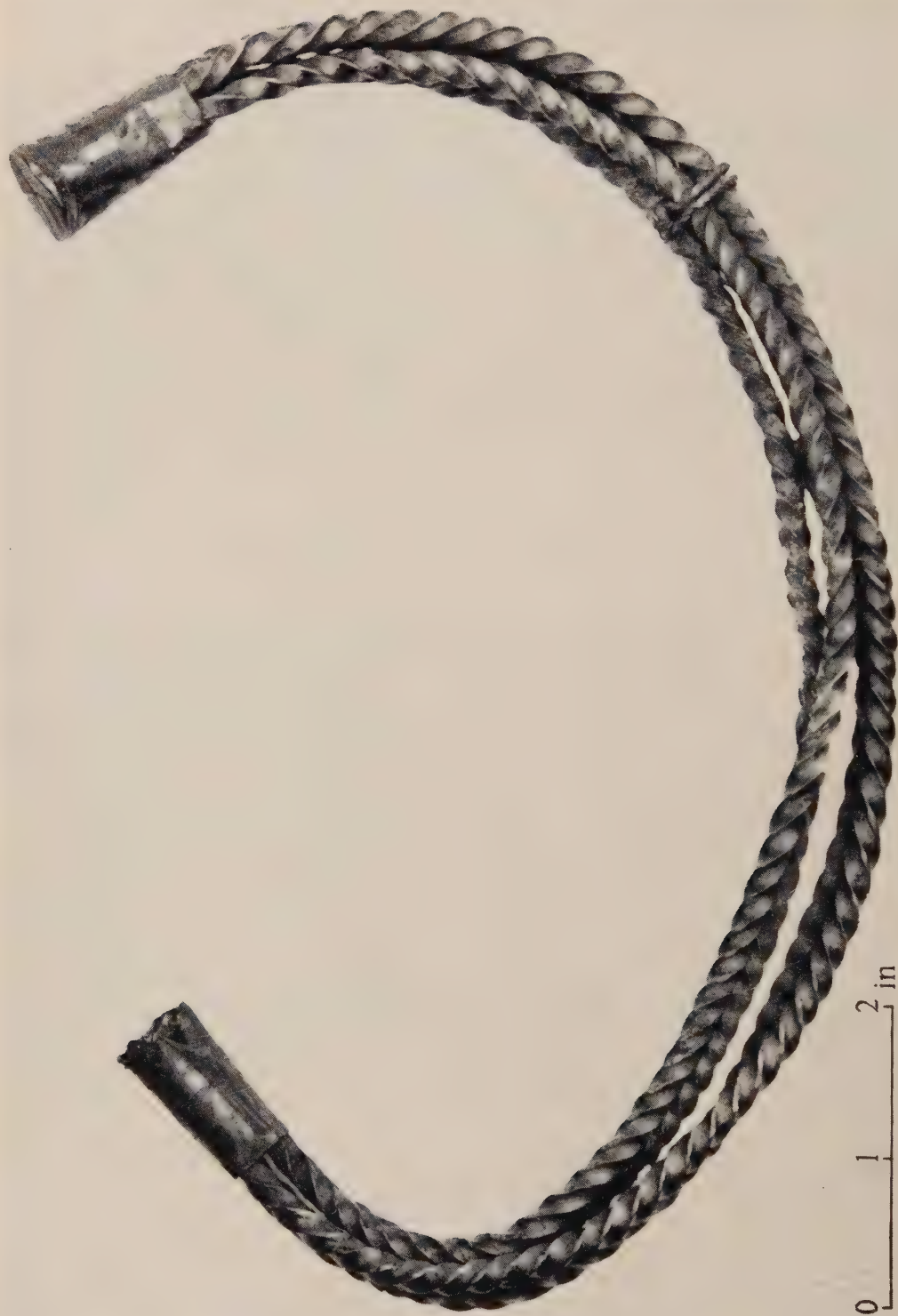
THE FIRST FARMERS IN TURKMENIA
Geoksyur style pottery. Late Namazga II.

PLATE XXIX



YANIK TEPE

Circle 6 (level 5), showing doorway (bottom right) and 'standard installations'.

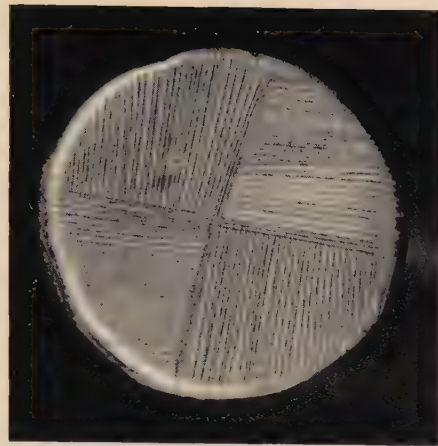


THE MOULSFORD TORC

A remarkable Bronze Age gold torc found in Berkshire in 1960.



(a)



(b)



(c)

THE MOULSFORD TORC

(a) Enlargement of the right-hand terminal in the photograph above; (b) the end-cap of the right-hand terminal; (c) enlargement of the section of the torc held together by 'a binding of spirally-twisted stout gold wire' (see p. 240).



ART IN ROMAN BRITAIN

Head of a Man (first century A.D.) from Gloucester.

See p. 246]

Photo : Gloucester City Museum.

WORSAAE AND BRITISH ANTIQUITIES

In a number of instances it is evident that the men who shared Worsaae's activating friendship were also members of the small, individualized, and at times discordant group struggling for the recognition of the value and importance of British antiquities. In the years before 1859 their work was limited generally to recording objects and providing examples to illustrate literary references. They had neither the theory of evolution to influence their speculations, nor the word 'prehistory' to describe their studies, yet they maintained that relevant historical information could be elicited from material remains. To these remarkable men, energetically committed to a cause that as yet commanded little general enthusiasm, Worsaae became adviser, confidant and example. His declared intention in travelling and writing was to provoke a more lively interest in national antiquities; his correspondence indicated how unflinchingly sympathetic he must have been towards a great diversity of inquirers. The reactions his words provoked and the ideas he stimulated were various; but they were alike in their vehemence, their enthusiasm and their optimism.

The critics of what was the current attitude towards British antiquities maintained as one of their foremost points the necessity for a national museum, or at least for a department of national antiquities in the existing institution. As Kendrick has described, they denounced the British Museum for neglecting the conservation and display of national antiquities for the sake of the more spectacular discoveries of the Near East. The postulated existence of such a department posed the considerations both as to what it should contain and how it should be arranged.

It is accepted that despite earlier imprecise anticipations of the idea among British antiquaries, it was Worsaae who introduced into this country the concept of the Three Age System, not only as a museum classification, but as a working hypothesis for prehistoric archaeology.⁶

In December 1846 Worsaae addressed the Royal Irish Academy on the theory of the three ages, indicating its relevance for the whole of Europe and discussing the nature of each period in terms of human societies. 'These implements of stone,' said Worsaae, referring to artifacts which would now be termed Neolithic, 'seem to belong to a time before all history, to an aboriginal people who lived by fishing and hunting upon the seacoasts or along the large rivers of Europe.' These men were the builders of the 'cromlechs' for these were 'raised by a people who had no metal, and therefore were unable to penetrate into the interior of Europe, which was then covered with forest and morass of an immense extent'. He ascribed the bronze objects 'to a new people who invaded Europe, following the larger rivers into the interior parts, where, with their instruments of metal, they were now able to make their way through the thick forests'. He added that it was these people who 'began to drain the bogs and cultivate the soil, no longer living merely by fishing and hunting, as the people before them'. Worsaae considered, however, that all the iron tools and weapons were post-Roman, and 'barbarized imitations of Roman designs'.⁷ A few years later he was even more explicit, and wrote that 'The same division of the antiquities into three classes—those belonging to the periods of Stone, Bronze and Iron—which has been adopted in the arrangement of the Danish primeval monuments, will apply to the British remains.'⁸

Worsaae's two lectures in Dublin are pregnant with information. The importance he assigned to the Irish monuments and his lucid account of the history and theory of archaeology as he saw it, must have provoked discussion among the audience and reinvigorated

⁶ Glyn Daniel, *A Hundred Years of Archaeology*, 38–54 and 77–85; Joan Evans, *A History of the Society of Antiquaries*, 280–1.

⁷ *Proceedings of the Royal Irish Academy*, III, 1845–7, 328–33.

⁸ *The Primeval Antiquities of Denmark*, VI.

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interest in national antiquities. One member of this audience was almost certainly Edward Clibborn, curator of the museum of the Royal Irish Academy, who wrote to Worsaae early in 1847 begging him 'Do stir the people up at the British Museum—knock their heads together.'⁹

Possibly Worsaae, by rather more delicate methods, had done something of the sort already. He records in his autobiography that he had met several members of the staff of the British Museum very soon after his arrival in London the previous June. Among them had been Samuel Birch, an Egyptologist then assistant to Edward Hawkins in the Department of Antiquities. During the spring of 1847 apparently Birch had been pondering on what he had heard about the conservation of antiquities in Denmark, for in May he wrote to Worsaae a long and carefully-worded letter containing thirteen detailed questions on the origin and organization of the National Museum in Copenhagen. Birch asked Worsaae for information about the labelling and cataloguing of objects, he enquired whether there was a separate collection of national antiquities and whence came the funds for purchases and the attendants' salaries; he even wanted to know if the Museums had 'exerted any marked social influence on the country'; but the most pertinent question is the sixth, which indicates that in Birch's mind at least archaeology was beginning gradually to be distinguished from art history. 'What principle of arrangement is adopted?' he asked, 'Are the large monuments arranged artistically, i.e., to suit the eye—or placed chronologically so as to illustrate the history of art, or classed according to their rank or employment?'¹⁰

Birch mentioned that there were stirrings within the British Museum and rumours of a Royal Commission to enquire into its condition. Meanwhile it was to Worsaae, spokesman of the first country to develop a conscience concerning her archaeological monuments, that the small group of enthusiasts turned. Their letters ranged from Birch's cautious and non-committal enquiries to a high note of despond from Roach Smith, who wrote that Mr Hawkins, when asked by the commissioners 'how it was that *experienced* antiquaries were not placed over the antiquarian departments instead of persons brought over from colleges, replied *there were no archaeologists in England*'.¹¹

In his preface to Thoms's translation of his book, probably written about this time, Worsaae summed up the general lack of interest and absence of system in the study of British antiquities. 'Whilst the antiquities of Rome, Greece and Egypt have been carefully examined and systematically described by English writers, the primeval national antiquities of the British islands have never hitherto been brought into a scientific arrangement,' he wrote, and later was more precise. 'I hope the day is not far distant when the British people will have formed a national museum of antiquities commensurate with the importance of their remains.'

The partisans were enthusiastic even if the establishment was cautious. Daniel Wilson in his book *The Archaeology and Prehistoric Annals of Scotland* (1851) referred frequently to Worsaae and to examples from Danish archaeology, and maintained that the Three-Age System 'was justly esteemed the foundation of Archaeology as a science'. It was a foundation

⁹ Unpublished letter in the archives of the Second Department of the National Museum, Copenhagen. I was able to study some of this material in July 1960 and am very grateful to Dr O. Klindt-Jensen, Dr G. E. Daniel and Mr D. M. Wilson for their advice and encouragement, and to members of the First and Second Departments for their kind assistance. I only regret that Mr Victor Hermansen's sudden death deprived this work of so much valuable criticism.

¹⁰ Unpublished letter in the National Museum, Copenhagen. I am very grateful to Mr D. M. Wilson of the Department of British Antiquities in the British Museum for looking for Worsaae's reply to Birch, unfortunately without success.

¹¹ Unpublished letter in the National Museum.

that was only to be built upon slowly. A guidebook to the British Museum in 1850 classified the exhibits as belonging either to the period of Druidism, or as Romano-British or Anglo-Saxon. Five years later a similar publication made a cautious reference to 'the Stone and Bronze Periods of the Northern Antiquaries'.¹² It was not until 1866 that a separate and specific department of British and Mediaeval Antiquities was created, and arranged by its first keeper, Augustus Wollaston Franks—apparently somewhat hesitantly—according to the Three Period System. Yet only twenty years later, and a year after Worsaae's death, Morland Simpson could write in the preface to his translation of *The Prehistory of the North* that the Three Age System had now become familiar to everyone, the terms were used without question, and 'it would even seem to require an effort of imagination for the younger generation to realise to whom we owe them, and how keen was the controversy which national antipathy raised upon this system of division'.

The opponents of its adoption in Britain were by no means uninformed or reactionary. They had a very proper fear of the too rigid application of a theory, a fear manifested all the more against the protagonists of evolution after 1859. The two most articulate critics were Thomas Wright and John Mitchell Kemble, both experienced and intelligent antiquaries. Wright, who was the more emphatic, declared that archaeology was a department of history, and as such 'cannot be treated as a physical science'. The northern antiquaries had made a generalization based upon inadequate evidence, he wrote, and formed a system 'specious and attractive in appearance, which a vain attempt has been made to introduce into this country'. The only valid method for the arrangement of a museum was according to the peoples to whom the objects had belonged.¹³ These opinions were published only a year after Daniel Wilson had given the public his very different ideas on what Wright called 'this vague system of metallic periods'. Kemble was milder in his criticism and admitted embarrassment at addressing the Irish Academy in opposition to Worsaae who had spoken on the same theme only ten years earlier. In a lecture given at the opening of the Hanoverian Museum and printed in *Horae Ferales* Kemble had written under the headings of the Three Ages; in his address in Dublin in February 1857, only a few weeks before his early death, he was more forceful. He considered the system was untenable because artifacts of one material were found in association with those allegedly characteristic of a subsequent period, though he admitted that he was 'perfectly aware of its value in the co-ordination and arrangement of a museum'.¹⁴ Wright also put forward the same argument, which was not justified. Worsaae's brilliant common-sense prevented him from applying his ideas too rigorously, and he had himself written that 'Bronze tools gradually supplanted the implements of stone, which however continued for a long time to be used by the poorer classes'.¹⁵ Wright was obdurate in opposition to what he considered to be an unnatural and un-historical theory; later he reinforced his arguments with an ingenious observation. 'Many of these implements of stone must have been made with metal tools,' he stated in 1861. 'It appears to me that even the common flint implements can only have been chipped with metal, and I suspect, moreover, that that metal was iron . . . it seems to me equally evident that most of these implements in stone were really copies of similar implements in metal'.¹⁶ Despite criticisms of Scandinavian archaeology that were occasion-

¹² T. D. Kendrick, *op. cit.*, 144.

¹³ Thomas Wright, *The Celt, the Roman and the Saxon*. 1852. His arguments were unchanged in the 3rd ed. of 1875.

¹⁴ J. M. Kemble, *The Utility of Antiquarian Collections as Throwing Light on the Pre-historic Annals of the European Nations*. Address to the Royal Irish Academy, February 1857.

¹⁵ *The Primeval Antiquities of Denmark*, 122.

¹⁶ Thomas Wright, *Essays on Archaeological Subjects*. 1861, 13.

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ally outspoken, both Wright and Kemble mentioned the increasing interest in national antiquities at the time, and Kemble concluded his last address with a sentiment very much in sympathy with Worsaae's view of archaeology as the most national of studies. 'Now, Gentlemen,' he said, 'let us, with the full spirit of an enlightened patriotism, devote ourselves to the illumination of our own antiquities.'

Apparently Worsaae's visit provoked, if only slightly and temporarily, the apathy of the public towards British antiquities. 'We have as good reason to be proud of our history as any people on earth,' retorted the *Edinburgh Review* in 1847, admitting, however, 'that the study of national antiquities has not fared so well with us as on the Continent.' Certainly in the years following Worsaae's first visit to Britain it was to Danish advice and example that the small group of enthusiasts turned, in more than one aspect of the question. They were striving not only for a national museum and for the adequate recognition of antiquities by existing bodies; they were also seeking a Parliamentary decision concerning archaeological monuments. They were working for the reform of the existing law of treasure trove and for the legal protection of field monuments. In 1846 Worsaae had related that Denmark had once had a law similar to the one in operation at that time in Britain. All chance discoveries of gold and silver had to be delivered to the Crown, without recompense to the finder, with the inevitable result that numerous finds were kept secret and the articles melted down. The Danish law had been modified under King Frederick V in 1752, by an ordinance decreeing that the finder who reported his discovery should receive payment of the full value of coins or any object 'which on account of age or otherwise is considered to be of rarity'.¹⁷ Worsaae's views on the deplorable results of the existing British law were given all the more point by an undated incident that may well have occurred on his first visit to Scotland, a time when his correspondence records that he was anxious to purchase antiquities. A certain individual offered Worsaae 'a splendid armet, a serpent coil, heavy and massive, the gold alone of which must have been worth at least £20; but for which he asked only a £10 note'. The article had been found during farming, and knowing he would get no reward if he proclaimed his discovery, he was hoping to dispose of it advantageously and in secret.¹⁸

Worsaae's remarks on treasure trove in his address given in Dublin were taken up at a meeting of the Royal Archaeological Institute in February of 1849; in their discussion on a hoard from a tumulus in Fifeshire, and some gold bracelets produced by Mr Dundas, members advocated an alteration of the law according to the Danish example.¹⁹ By the 1850s the small group of antiquaries interested in their native archaeology had begun very definitely to feel that their opinion required a legal expression, and that public interest might be sufficiently aroused to effect the necessary measures. In July 1851 Edward Hawkins wrote to Copenhagen asking for information about the Danish law concerning Treasure Trove.²⁰ Henry Rhind, whose two papers on the law and antiquities embodied exhaustive research and intelligent suggestions, was more demanding. In a letter written in August 1855, he required 'an accurate view of the whole system of antiquarian supervision existing in Denmark, in its various ramifications'.²¹ Rhind quoted Worsaae *verbatim* in his paper on treasure trove and used Danish examples extensively in both articles.²²

¹⁷ Sir George Hill, *Treasure Trove in Law and Practice from the Earliest Times to the Present Day*. 1936, 182.

¹⁸ Horace Marryat, *A Residence in Jutland, the Danish Isles and Copenhagen*. 1860, 252. Chap. xvi is an interesting and detailed account of the National Museum in Copenhagen at this time.

¹⁹ *The Athenaeum*, no. 1112, 171.

²⁰ Unpublished letter in the National Museum.

²¹ Unpublished letter in the National Museum.

²² A. H. Rhind, *British Antiquities Their Present Treatment and their Real Claims*. 1855.

A. H. Rhind, *The Law of Treasure-Trove*. 1858.

WORSAAE AND BRITISH ANTIQUITIES

Their cause found a champion. Lord Talbot de Malahide, who was a collector of Roman and Irish antiquities, a member of the Irish Academy and later its President, introduced a bill into the House of Lords in July 1858 stipulating that the finder of treasure trove should receive the full antiquarian value of the articles. The bill was abandoned owing to difficulties raised by the Treasury, and it was not until 1886 that a Minute issued from the department authorized the payment of a large percentage at least of the value of an object to its finder. Scotland and Ireland were a long way in advance in this reform, making similar measures of amendment in 1859 and 1861 respectively.

The question of the protection of field monuments was a more vexed one. Here again Danish precedent was sought. 'We are anxious,' wrote John Lubbock in an undated letter, 'to know what has been done in your country to prevent the destruction of tumuli, dolmens, and other similar monuments.'²³ He felt there would be a favourable influence on Parliament 'if they felt that they were following the example of Denmark', and Danish precedent was referred to frequently in the many and prolonged debates on Lubbock's bill. First proposed in 1871, the measure suffered remorseless subversion again in 1877 and 1878. The members were obviously disquieted at the thought of possible outrage to landed interests and proprietary rights, and when eventually the bill reached the Lords two years later the opposition 'did not show that power of calm argument by which the debates of the Upper House are supposed to be distinguished'.²⁴ In February of 1879 the Society of Antiquaries of London, who had been drawing up a list of sepulchral monuments worthy of protection, decided to support Lubbock's bill, after they had heard Franks read a paper by Worsaae on the protection of monuments in Denmark to supply them with some comparative information.²⁵ After amendments and vicissitudes, ultimately the bill became law in 1882, with Pitt Rivers as the first Chief Inspector of Ancient Monuments.

In these definite and recorded measures it is possible to discern Worsaae's formative guidance of those antiquaries whose energies were directed towards establishing British archaeology. In rather less tangible ways Worsaae's influence was greater and more penetrating. As Sophus Müller states in his obituary note,²⁶ Worsaae did more than merely publicize the Three-Age System. He had extraordinarily modern ideas on the purpose and recording of an excavation and he was the first archaeologist to undertake journeys specifically in order to study the archaeological material in other regions. As a result of these travels he had at his disposal a body of comparative material from which deductions and generalizations of hitherto unprecedented scope might be made. Müller emphasizes that Worsaae was the first scholar to subject archaeological material to a critical and entirely objective examination, and this method justifies his claim to be called the founder of prehistoric archaeology as a science. These ideas and information were made available through a correspondence of literally world-wide extent, which included such diverse individuals as Schliemann and W. E. Gladstone. Numerous British archaeologists were beneficiaries of Worsaae's experience and advice from 1846 until his death in 1885. Though he did so much to encourage interest in national antiquities, Worsaae opposed insularity and pointed out on his first visit to Britain that comparison between the antiquities of different countries was the only method of achieving answers to archaeological problems. Even J. M. Kemble, in other matters so critical of Danish ideas, bade his

²³ Unpublished letter in the National Museum.

²⁴ C. P. Kains-Jackson, *Our Ancient Monuments and the Land Around Them*. 1880. A list of the prehistoric monuments it was proposed to schedule under Lubbock's bill. Appendix A gives some details of the debates, and names of supporters and opponents at the division on the second reading in March 1877.

²⁵ Joan Evans, *A History of the Society of Antiquaries*. 1956, 330-3.

²⁶ *Mémoires de la Société Royale des Antiquaires du Nord*. N.S. 1884-9, 143-192.

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fellow antiquaries in his last address to consider the monuments of their own land 'only as links in one great chain, which embraces many nations'.

The fertile nature of this contact, through Worsaae, between archaeologists working in different countries, may be illustrated by two letters, which inter-relate the methodological progress of the Scandinavians and the recognition of the palaeolithic artifacts on the Somme with the intelligent observation that was characteristic of the best tradition of British field archaeology. In March, 1862, what Joan Evans has described as 'the axioms of prehistory' were set out cautiously in a letter from Worsaae to John Evans. Worsaae must have been revising his earlier concepts in the light of recent changes in thought. He suggested that the crude stone tools recognized in some British caves and in the French river terraces must have belonged to a certain peculiar race, who were succeeded by hunter-fisher peoples, who spread along the coasts to settle on islands and besides lakes and rivers. Finally, 'a higher civilization with domestic animals, with agriculture and with better formed implements had been introduced, perhaps by new invasions'.²⁷

John Evans replied at great length two months later. He emphasized the geological evidence for the extreme antiquity of the artifacts from the Somme. Discussing the divisions of the Stone Age that Worsaae had outlined, he distinguished between a period characterized by the rough implements found at Brixham and Kent's Hole and on the Somme, and the time of the reindeer hunters in South-West France. Wisely he did not commit himself on the question of a peculiar race, and suggested a system of relative dating by means of the associated fauna. He avoided uncritical generalization with patient good sense, and reminded Worsaae 'that it is extremely difficult to correlate the remains of the same character which are found in different vicinities . . . it would be very unsafe to assume anything like a synchronism of the Stone, Bronze and Iron periods of different countries'.²⁸

Worsaae's concept of the importance of archaeology as a distinct and valid discipline was unqualified. His possibly ingenuous view of its political significance saw archaeology as a source of national pride in small and oppressed countries, among which he included Scotland and Ireland, and as itself the outcome of the respect for the rights of the people that was a legacy of the French Revolution. He traced the interest in national monuments to Lenoir's *Musée des Monuments Français* (1800-6) which is in all respects an art history, though it includes Celtic antiquities and advocates a chronological arrangement of museum exhibits.

The magnitude of Worsaae's subsequent reputation was the result of his own energy, self-confidence and intelligence. He became a national figure in Denmark, and the impression he gave to an Englishman some twenty-five years after his first visit to Britain is described by Edmund Gosse, who met him at a dinner-party in Copenhagen. He wrote that 'Worsaae had the *allures* and the appearance of a statesman. Imposing in manner, even Olympian, accepting graciously and as a matter of course his own pre-eminence. . . .'²⁹ The eloquence of this description is commensurate with Worsaae's position in the history of the development of European archaeology.

²⁷ Joan Evans, *Time and Chance*. 1943, 105-6.

²⁸ Unpublished letter in the National Museum. I am very grateful to Dr Joan Evans for her comments.

²⁹ Edmund Gosse, *Two Visits to Denmark*, 1872 and 1874. 1911, 251-7.

Settlement Patterns in Anglo-Saxon England

by GLANVILLE JONES

Mr G. R. J. Jones is a Welsh-speaking native of Gower who has been a Lecturer in Geography in the University of Leeds since 1949. He has for many years been engaged on a full-scale study of the relationship between settlement patterns and land tenure in early Wales. In this article he examines the basic patterns of settlement distribution in Anglo-Saxon England.

THE scientific study of rural settlement in Britain was initiated in 1883 when Seebohm published *The English Village Community*. In this study he argued for a continuity between the Roman villa and the Anglo-Saxon village.¹ In 1897, however, Maitland published his *Domesday Book and Beyond*² by way of rejoinder to Seebohm's views. Thereafter it became customary for historians, geographers and archaeologists alike to regard the nucleated settlements of the English lowlands as Anglo-Saxon creations, implanted by groups of free English settlers in a countryside emptied, by force or fear, of its Romano-British occupants. Of late this hypothesis, which lays so much stress on Germanic as distinct from earlier achievements, has come under criticism from several quarters. Archaeologists are increasingly conscious of the survival into the Dark Ages of a British tradition in ornamentation even in England.³ As a philologist, Jackson has adduced sufficient linguistic evidence to undermine, even for eastern Britain, the old theory of the near-complete extermination of the Celts and has argued for a bilingual phase when English settlement was being effected.⁴ On a narrow front in the Cotswolds the historian, Finberg, has cogently portrayed various facets of continuity between the Roman villa and the adjoining village at Withington.⁵ On a broader front, Aston has demonstrated the limitations of Maitland's thesis on the origin of the manor; he suggests that the primary Anglo-Saxon penetration of England was carried out not by freemen, each owning one hide of land, but under lordly direction and was therefore organized by large estates.⁶ The vacuum created by this sapping of the traditional view of Anglo-Saxon settlement has not been filled by the adoption of a convincing alternative hypothesis.⁷ This absence of an alternative is largely due to fundamental misconceptions about the organization of that Celtic society which occupied England before the advent of either Romans or Saxons. The purpose of this article is to remove these misconceptions and, by so doing, to show that the basic patterns of settlement distribution in the English lowlands date from at least Celtic times.

¹ F. Seebohm, *The English Village Community*, 1883.

² F. W. Maitland, *Domesday Book and Beyond*, 1897.

³ D. B. Harden (ed.), *Dark-Age Britain*, 1956, 16-39, 112-22.

⁴ K. Jackson, *Language and History in Early Britain*, 1953, 235.

⁵ H. P. R. Finberg, *Roman and Saxon Withington*, 1955.

⁶ T. H. Aston, 'The Origins of the Manor in England', *Trans. Roy. Hist. Soc.*, New Series, VIII, 1958,

74-5.

⁷ Cf. J. N. L. Myres in *A Survey and Policy of Field Research in the Archaeology of Great Britain*, I, 1948, 115, 'We need to know far more than we do about the date at which, and the means by which, the pattern of village distribution in different parts of the country took its historic form'.

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The most important of these misconceptions arose when Seebohm claimed that, from pre-Roman times onwards, the history of settlement in England had proceeded along quite distinct lines from that in Wales. He envisaged for the Celts a social organization, a tribal system, based on kinship and an economy based overwhelmingly on semi-nomadic pastoralism. In these beliefs he was followed by all subsequent investigators. Nor were such views unpalatable to most Welshmen for, well versed in Biblical tradition, they were not unwilling to equate their oldest traditions with those of the Hebrews of old. Thus medieval Wales was regarded as a late surviving Wild-West inhabited by footloose Celtic cowboys. The scattered farmsteads of modern Wales, so appropriate for a pastoral economy, came to be accepted as characteristic of Celtic settlement through the ages.⁸ Consequently, archaeologists have tended to interpret Iron Age settlements in the light of modern Celtic parallels. Added impetus was given to this tendency when Bersu, after excavating Little Woodbury, Wiltshire, interpreted the so-called pit dwellings of this and other Iron Age sites as grain storage pits, and argued that Little Woodbury, far from being a hamlet as earlier workers had assumed, was but a lone stading.⁹ Hawkes, by his re-examination of similar sites, like Woodcuts and Rotherley in Cranborne Chase, confirmed this interpretation.¹⁰ As a result, Piggott has recently argued that the 'Woodbury type of economy' in Wessex and Sussex during the Iron Age is marked by 'settlement units of single farmstead, or Einzelhof, type rather than villages'¹¹; and Rivet has gone so far as to assert that 'the single farm, whether constructed in timber, as in Wessex and Sussex, or in stone, as in the West and North, was the normal form of rural habitation, throughout the Iron Age'.¹² Iron Age villages like All Cannings Cross, Wiltshire, and smaller nucleated settlements like Chysauster in Cornwall have presumably therefore come to be regarded as exceptions. Under such circumstances it is hardly surprising that Jackson, in his recent study of Early Britain, could write that 'British villages were small and scattered, of the "non-nucleated" type, which would not suit the English with their nucleated villages'¹³; partly for these reasons, he regarded the survival into the Dark Ages of settlements inhabited by Celts as the exception rather than the rule in England.

The traditional view of Celtic social organization was based on the early epic literature and law codes of Ireland and Wales. These sources reveal a warrior society in which wealth was counted in cattle and heroic exploits centred on cattle stealing. But, for Wales, administrative records of the Middle Ages make it clear that this was true only of a ruling caste, a thin aristocratic crust. Bondmen, whose economy had a stronger arable bias than that of their overlords, constituted an unfree majority. These bondmen resided in small hamlets, each containing about nine houses, but small though they were these hamlets were nevertheless nucleated settlements. The hamlet was the characteristic early form of settlement, for the scattered single farmstead became a prominent feature of the Welsh countryside only in the later Middle Ages.¹⁴

⁸ For example, A. Fox, 'Celtic Fields and Farms on Dartmoor', *Proc. Prehist. Soc.*, New Series, xx, 1954, 95.

⁹ G. Bersu, 'Excavations at Little Woodbury, Wiltshire', *Proc. Prehist. Soc.*, New Series, vi, 1940, 30-111. Cf., however, L. Scott, 'Corn-drying Kilns', *ANTIQUITY*, 1951, 205-6, who regards the pits as providing 'storage for other foodstuffs, notably the quantity of air-dried or salted meat which the farm must have needed to preserve after the Michaelmas slaughter of the beasts which could not be kept alive over the winter'.

¹⁰ C. F. C. Hawkes, 'Britons, Romans and Saxons', *Arch. J.*, ci, 1947, 36-48.

¹¹ I. A. Richmond (ed.), *Roman and Native in North Britain*, 1958, 3.

¹² A. L. F. Rivet, *Town and Country in Roman Britain*, 1958, 40. Cf. however, the review by S. S. Frere in *Past and Present*, No. 16, 1959, 3.

¹³ K. Jackson, *op. cit.*, 235.

¹⁴ G. R. J. Jones, 'Medieval open fields and associated settlement patterns in North-west Wales', *Géographie et Histoire Agraires*, 1959, 313-28; 'Rural Settlement: Wales', *Advancement of Science*, No. 60, 1959, 338-42.

[note continued p. 223.]

SETTLEMENT PATTERNS IN ANGLO-SAXON ENGLAND

Each hamlet community cultivated a small area of open field and grazed common pastures, parts of which were periodically subject to temporary outfield cultivation. The hamlet was the local unit of agrarian co-operation, but at the same time it was part of a wider grouping, originally known as a *maenor*, which was subject to the authority of a lord. In return for rents and services to this lord, the bondmen in Welsh society held equal shares in that soil to which they were bound. These obligations included grain and cattle rents, the feeding of the lord and his retinue at certain seasonal circuits, the construction of the lord's court or hall (*llys*) and the cultivation of the lord's demesne lands. The demesnes were normally situated on the best soil within the area subject to the lord's control; since they were cultivated for the maintenance of the court, this too was situated nearby, on the outskirts of a bond hamlet sometimes known as a mayor's settlement.¹⁵

For North Wales it has been demonstrated that some bond hamlets were in being long before the earliest English colonization was effected along the border. A few of these mayor's settlements, or adjacent lord's courts, bore the names of adjoining hill-forts and the majority of these were located in the lowlands beneath these forts. Dinorben, in Denbigh, is an interesting case in point.¹⁶ A centre of importance in the Dark Ages, it was a manor and the *caput* of the commote (administrative neighbourhood) of Rhos Isdulas in 1334; the construction of the court buildings was a duty borne by the bondmen of five hamlets appendant to Dinorben and situated within this commote. The medieval court was sited to the south of the hill-fort but in earlier centuries was probably within the ramparts, where the spade of the archaeologist has revealed not only traces of numerous small huts, but also the remains of a succession of more substantial structures.¹⁷ One of the latter was a circular structure of Little Woodbury type and dated to the third and fourth centuries A.D., when it was inhabited, or so numerous coins would suggest, by an affluent household. In later layers, possibly of the fifth or sixth centuries A.D., there is evidence to suggest a large rectangular hall of the kind described in the Welsh records as characteristic of a lord's court. The medieval courts of Wales were sometimes encompassed by protective walls and we can therefore envisage that the ramparts of a hill-fort served a similar purpose at an earlier stage of Celtic social development. If so, Dinorben provides a clear answer to that question which has for so long puzzled archaeologists; what kind of social organization made possible the construction of such a substantial structure as a hill-fort? An undertaking of this kind would have required control over the resources of an area far larger than the lands of one settlement. The rents and labour services levied by the lord of Dinorben on the tenants of the appendant hamlets of his neighbourhood provide testimony as to how the requisite control over the resources of a wide area was effected. Moreover, some of these hamlets, to judge from the evidence of an asymmetrical winged ploughshare—devised for the cultivation of heavy lowland soils—which was found in a late Roman context within

¹⁴, note continued from p. 222.]

It is particularly unfortunate that students of settlement form have tended to classify both isolated farms and small groups of ten or so farmsteads as 'dispersed settlements', and only larger groups of farmsteads as 'nucleated settlements'. The notions of nucleation and communal organization are far more important criteria than mere considerations of size. Since both notions underlay the formation of Welsh hamlets, these are regarded as nucleated settlements in this article.

¹⁵ G. R. J. Jones, 'Some Medieval Rural Settlements in North Wales', *Trans. and Papers* 1953, *Inst. of Brit. Geogs.*, 1954, 66-9.

¹⁶ G. R. J. Jones, 'The Pattern of Settlement on the Welsh Border', *Ag. Hist. Review*, viii, 1960, 66-81.

¹⁷ H. N. Savory, 'Excavations at Dinorben Hill Fort, Abergele (Denbs.)', 1956-7', *Bull. Board of Celtic Studies*, xvii (1958), 296-309; 'The Excavations at Dinorben Hill Fort, Abergele, 1956-9', *Denbighshire Hist. Soc.*, viii, 1959, 18-39.

the Dinorben hill-fort,¹⁸ were already by early centuries A.D. established on the same fertile lowland sites they occupied in the Middle Ages.

In medieval England there were numerous discrete estates, or federal manors, each made up of one central settlement, often a *villa regalis*, containing the lord's court (*aula* or *mansio*), and a number of appendant hamlets. In all essentials the discrete estate resembled the Welsh *maenor*. Appropriately enough, analysis of settlement along the Welsh border has revealed that a relationship akin to that at Dinorben can be demonstrated between Iron Age hill-forts and discrete estates recorded on both sides of Offa's Dyke in 1086. Study of Northern England from a slightly different standpoint has revealed not a few lowland hamlets bearing names like Walton, Cumberworth or Bretton, which are all indices of permanent settlement on the part of an old-established Welsh population. In the Middle Ages these hamlets almost invariably formed part of discrete estates, the shires of Northumbria, whose central settlements occupied sites standing in a significant relationship to hill-forts or major Roman settlements. Thus a similar organization to that found in early Wales can be envisaged even for the easternmost parts of Northern England.¹⁹ In parts of Southern England, likewise, a similar organization can be detected as a social substratum. Curiously enough, in view of Seebohm's denial of a Celtic basis for rural settlement in Eastern England, the village of Hitchin, in Hertfordshire, which he chose to illustrate his thesis of Roman-Saxon continuity, was the central settlement of a discrete estate closely resembling the Welsh *maenor* and the Northumbrian *shire*.²⁰ Hitchin, the head of a hundred, and an important old settlement, whose lands ultimately came to be worked on a three-field system, had a number of appendant hamlets; one of these lay beyond the county boundary in Bedfordshire, but the most worthy of note for present purposes is the hamlet of Walsworth—originally known as Walton's ford—whose name implies that it was in the distant past a settlement inhabited by 'serfs or Britons'.²¹ All this suggests that the discrete estate, comprising a number of lowland hamlets, was the fundamental basis of social organization alike in the Highland and Lowland Zones of Britain before the advent of either the Romans or the Saxons.

As in England north of the Tees, so in Hertfordshire earlier workers have advanced suggestions concerning a heritage of Celtic settlement, albeit along vague and inconclusive lines. Accordingly, emphasis in this paper will be directed towards two districts in Southern England for which historians have emphatically denied any Roman-Saxon continuity; these are the chalklands of Sussex and Wessex, of which Myres wrote that the Anglo-Saxon conquest 'involved a complete break with the agricultural past'.²²

Along the coastlands and South Downs of Sussex, pagan Saxon burials are numerous²³; so too are early English place-names in *-ingas*, which reveal a personal relationship, namely 'the dependants and followers of a certain individual'.²⁴ Together these betoken an intensive and early Anglo-Saxon occupation. In this same general area are to be found all the Roman villas located in Sussex and here too both Iron Age hill-forts and the central settlements or capitals of discrete estates are most numerous (FIG. 1). That the latter are old established

¹⁸ F. G. Payne, 'The British Plough: Some Stages in its Development', *Ag. Hist. Review*, v, 1957, 77-9.

¹⁹ G. R. J. Jones, 'Basic Patterns of Settlement Distribution in Northern England', *Advancement of Science*, No. 72, 1961, 192-200.

²⁰ F. Seebohm, *op. cit.*, 1-13, 424-35, 443-53.

²¹ *Domesday Book*, 1, f. 132b. J. E. B. Gover, A. Mawer and F. M. Stenton, *The Place Names of Hertfordshire*, 1938, 10-11.

²² R. G. Collingwood and J. N. L. Myres, *Roman Britain and the English Settlements*, 1936, 441.

²³ G. J. Copley, *An Archaeology of South-East England*, 1958, 152-176.

²⁴ A. H. Smith, *English Place-Name Elements*, 1, 1956, 300.

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is implied not only by their location in relation to the hill-forts but also by the fact that not less than ten of them bore names in *-ingas*. Further north, on the heavy clays and sterile sands of Northern Sussex, where hill-forts are fewer in number, few capitals of discrete estates were recorded in 1086. In some parts of the south too, and especially the extensive tract of fertile intermediate loams on the Sussex Levels near Selsey Bill, discrete estates were surprisingly few in number; but this is in part a reflection of the territorial structure

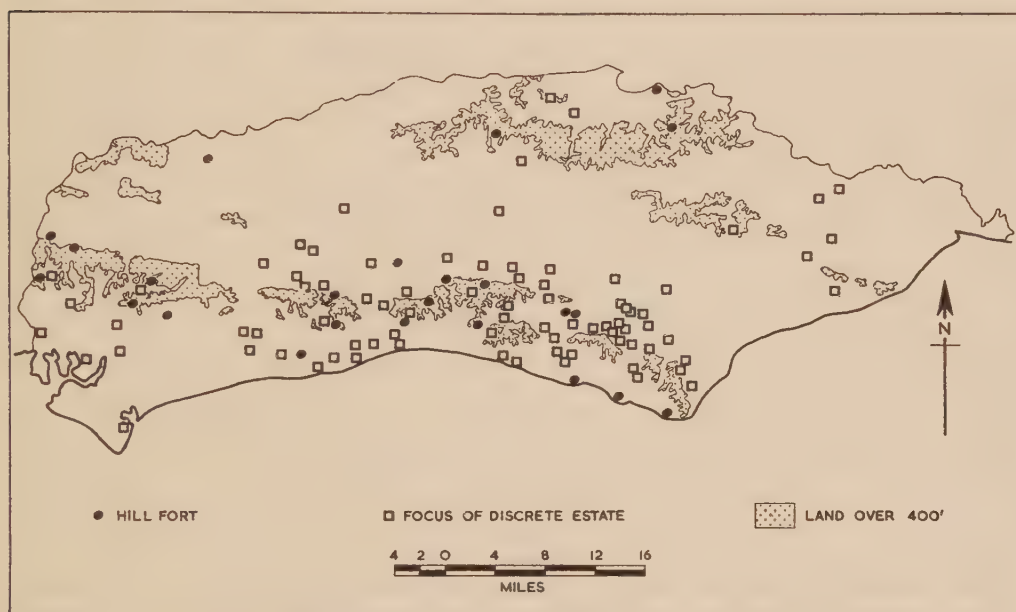


Fig. 1. Hill Forts and the Foci of Discrete Estates in Sussex.

of the ecclesiastical estate centred on Selsey. This federal manor contained numerous hamlets—the *villae sibi cohaerentes* of early charters—some of which were located in a fairly compact group to the north of the *caput*.²⁵ Most of the discrete estates of Sussex contained hamlets at a considerable distance from the central settlement and were often separated from the capital by the lands of other discrete estates. Thus the Findon estate contained a number of outlying hamlets. One was Tottington, five miles distant from Findon, at the foot of the chalk scarp beyond the Adur river; and there were also other hamlets which lay over eight miles west of Findon beyond the Arun.²⁶ Findon, itself the capital of this discrete estate, stood in the lowlands beneath the vast hill-fort of Cissbury Ring which, interestingly enough, like Dinorben, was re-occupied in the sub-Roman period. It has been estimated that the builders of Cissbury Ring excavated some 60,000 tons of chalk and employed 8,000–12,000 timbers for the purposes of revetment. Accordingly this hill-fort has been regarded as the capital of a tribe, whose members, it was suggested, grazed or cultivated that block of downland, covering some 35 square miles, which extends between

²⁵ J. W. Birch (ed.), *Cartularium Saxonicum*, I, 1885, 99; J. M. Kemble, *Codex diplomaticus aevi Saxonici*, II, 1840, 341.

²⁶ *Domesday Book*, I, f. 28.

the Arun and the Adur.²⁷ But, as is implied by the territorial make-up of the Findon estate, it is far more likely that the territorial base which made possible this constructional feat embraced a much wider, though discontinuous, area and a greater variety of resources.

As Payne has shown, Iron Age husbandmen were technically equipped to deal with the heavier soils of the lowlands²⁸; the siting of the capitals of discrete estates in the lowlands adds further force to his interpretation. Cereals were undoubtedly obtained therefore not only from the Celtic fields of the Downs—probably marginal land in any event—but also from the much more productive lowland loams.²⁹ Many of the discrete estates of Sussex extended into the Weald, where no doubt forest pasture rights were predominant, but where also some permanent settlement had already been effected before the dawn of the Christian era, as the occasional hill-fort indicates. Such control over a diversity of resources, obtained from a wide area, helps to explain how it was that a number of major hill-forts could be erected simultaneously on the South Downs.

As a result, the capitals of discrete estates were sited fairly close together in Southern Sussex. Aston, however, was misled by the close grouping of *-ingas* settlements on the Sussex coastlands into the belief that little room was left for more extensive groupings; and that as a result '*-ingas* settlements must originally have been unitary, estate and village, manor and vill being one'.³⁰ But, as the argument advanced in this paper indicates, the discrete estate was already old when the first Anglo-Saxons arrived and this state of affairs, of itself, helps to explain the servile substructure of those ancient estates in England which Aston has so convincingly reinterpreted.

Since the Anglo-Saxon conquest of Wessex took place almost a century later than that of Sussex, it could be anticipated that a form of continuity would here be immediately apparent from the disposition of hill-forts and discrete estates. Yet even when heads of hundreds—the hundredal manors which retained some of the administrative functions of discrete estates³¹—are taken into account, the relationship between hill-fort siting and the administrative arrangements of later centuries is less clear than in Sussex (FIG. 2). This, however, is simply due to the manner in which the estates of Wiltshire are described in Domesday Book. The appendant hamlets of discrete estates are described only incidentally and many very large estates, such as Salisbury, for which no hamlets are recorded, must have embraced a considerable number of such appendages. As Maitland observed, in 1897, 'when more than five-and-twenty teamlands or thereabouts are ascribed to a single place, we shall generally find reason to believe that what is being described is not a single vill'.³² There were some thirty such large units, most of which were centred in the vicinity of hill-forts. Close examination therefore reveals that the argument advanced for Sussex is equally valid for Wiltshire.³³

²⁷ *A Survey and Policy of Field Research in the Archaeology of Great Britain*, 1, 1948, 73; V. G. Childe, *Prehistoric Communities of the British Isles*, 1947, 198–9.

²⁸ F. G. Payne, 'The Plough in Ancient Britain', *Arch. J.*, CI, 1947, 82–111; *Yr Aradr Gymreig*, 1954, 13–40.

²⁹ For the distribution of these soils see S. W. Wooldridge and D. L. Linton, 'The Loam Terrains of Southeast England and their relation to its Early History', *ANTIQUITY*, 1933, 297–310.

³⁰ T. H. Aston, *op. cit.*, 75.

³¹ H. M. Cam, *Liberties and Communities in Medieval England*, 1944, 64–90.

³² F. Seebohm, *op. cit.*, 17.

³³ Brokenborough, in North-West Wiltshire, is a particularly interesting example of a large discrete manor associated with a fortification. This place was once known by the Celtic name for the fortification, to which a Saxon suffix was added. In due course the hybrid name *Kairdurberg* was replaced by *Brokenberg*. See J. E. B. Gover, A. Mawer and F. M. Stenton, *The Place-Names of Wiltshire*, 1939, 53–4; and *Victoria County History, Wiltshire*, 1, 1957, 50, 68.

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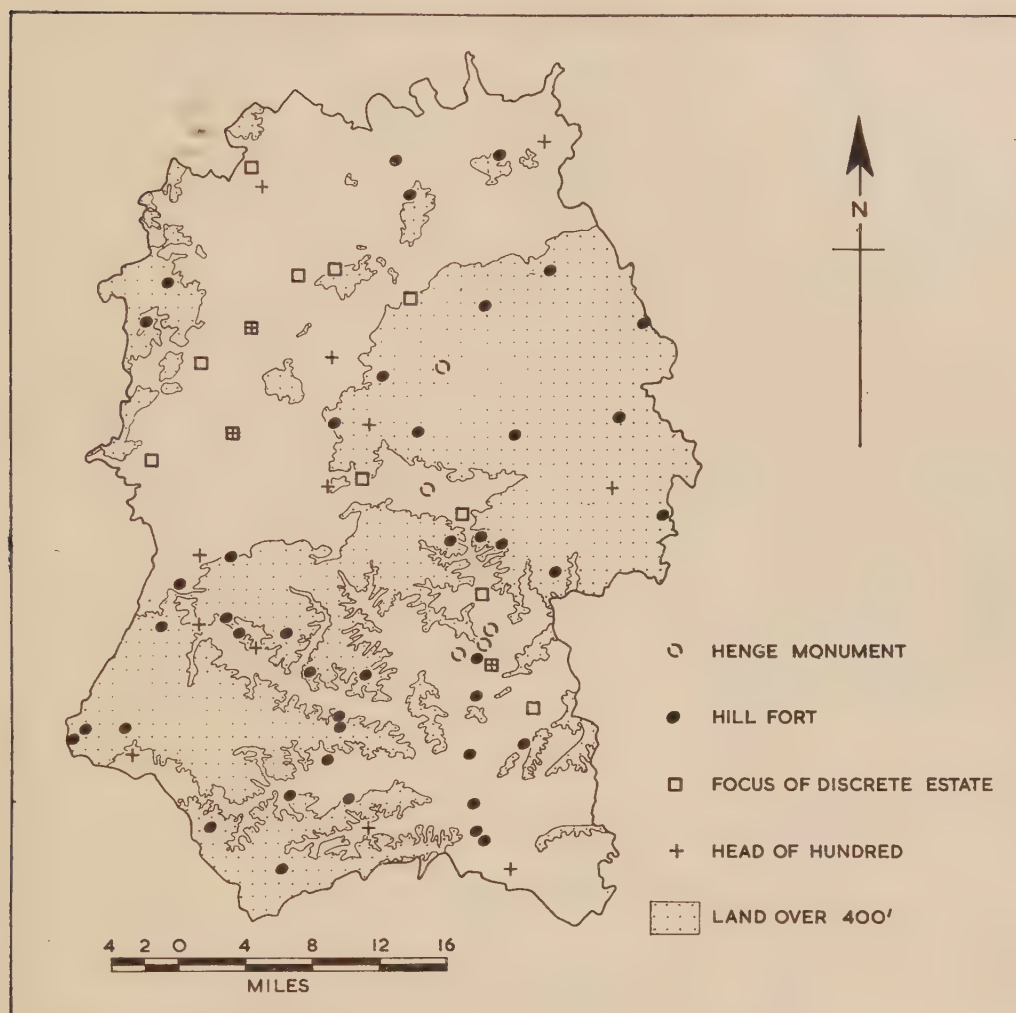


Fig. 2. Hill Forts and the Foci of Discrete Estates in Wiltshire.

The district of All Cannings in Wiltshire provides an interesting example. Here the original lowland settlement was probably at All Cannings, formerly Old Cannings, but the *caput* of Cannings Hundred came to be centred in the neighbouring village of Bishop's Cannings. Close at hand lies the Iron Age settlement at All Cannings Cross which seems to have been a village on a lowland slope, 'a situation remarked by the excavators as rather unusual for this part of the country where nearly all the known sites of ancient settlements are on the high chalk down'.³⁴ Since it occupied a site similar to that of All Cannings and of Bishop's Cannings, it is possible that these villages, which remain living entities, were also established in the Iron Age and therefore, like All Cannings Cross, stand in a significant relationship to the nearby hill-fort known as Rybury Camp.

³⁴ G. Bersu, *op. cit.*, 99-107.

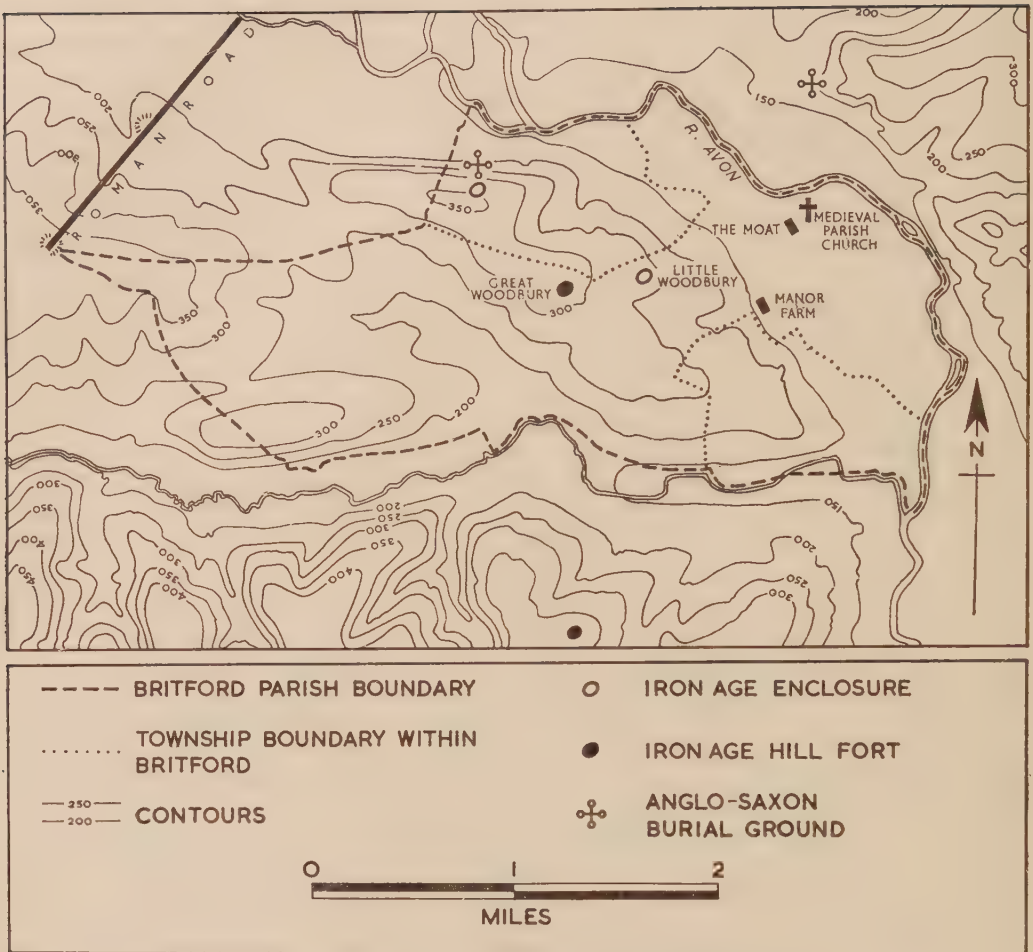


Fig. 3. The site and setting of Little Woodbury, Wiltshire.

A not dissimilar relationship appears to be in evidence in the parish of Britford where stands the Iron Age site of Little Woodbury, which was occupied from the beginning of the third century to the first century B.C. Bersu interpreted this site as the lone steading of an 'individual farm' of 20 acres, which was cultivated for cereals, but the position of the steading on chalk, far from water, does not entirely favour this interpretation (FIG. 3). As was the case with the hut of Little Woodbury type within Dinorben hill-fort, there is evidence of a certain rustic affluence at this Wiltshire site. The fact that a beginning was made with the provision of defences for the successive houses at Little Woodbury does suggest that an alternative explanation is possible. Bersu appears to have been influenced markedly in his interpretation by the limited storage capacity for grain in any one year at Little Woodbury, and therefore suggested that the products of other farms could not have been stored here.³⁵ Yet, as the parallel with Dinorben suggests, if Little Woodbury was

³⁵ *Ibid.*, 106.

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the residence of a Celtic territorial lord a large grain storage capacity would not have been needed; for ambulatory feeding of the lord, his retinue, horses, dogs and even his hawks was a characteristic feature of early Welsh social organization.³⁶ Curiously enough, Bersu, despite his final interpretation, does appear to have had in mind the labour services rendered on the central settlement of a discrete estate when, in 1938, he speculated about the social organization of Little Woodbury. He wrote: 'It was not a village but was manifestly inhabited regularly only by the proprietor of one or both large farms, and must be ranked as a farm, a forerunner of the later Roman villas. In the neighbourhood of the farmhouse, and protected in common with it by ditches and palisades, the stock and products of agriculture were preserved, partly in the granaries above the ground, partly in the pits. If our explanation of the hollows as work places is correct, then in the harvest season more people must have gathered at the manor to do the extra work.'³⁷ He added that 'to look for the smaller country houses or villages, in which these people normally settled, is a further task'. Some additional evidence for the Britford area, when taken in conjunction with that already cited for Dinorben and All Cannings Cross, indicates that the places where these Iron Age folk really settled were lowland hamlets.

One such hamlet, even in the Iron Age, probably stood near the site of Britford parish church, a favoured peninsula of valley gravel in the flat-floored trench of the River Avon. Just as the Avon was named by Celtic settlers, so too Britford appears to have derived its name from the ford used by the Britons. Moreover, it is fairly certain that Britford belonged to the same territorial unit as Little Woodbury, and the adjacent hill-fort of Great Woodbury, for a charter of *circa* A.D. 670 records that the bounds of the people of Britford—*Brytfordingea landscaere*—extended as far west as the point at which the present parish boundary meets the Roman road.³⁸ Appropriately enough, the royal manor of Britford in 1086 contained twenty teamlands and was almost large enough to merit consideration as a possible discrete estate.³⁹

For Amesbury, which is about two miles east of Stonehenge, there is some evidence to suggest that such lowland hamlets, and the discrete estates of which they appear to have formed part, date from a still more remote past. Like Britford, Amesbury stood on the bank of the same River Avon and was centred on the largest patch of valley gravel in the vicinity. Domesday Book records, in two distinct places, that the manor of Bowcombe in the Isle of Wight, and therefore at a distance of about 40 miles, had been a dependency of Amesbury, which was 'of the king's farm'⁴⁰ (FIG. 4). The only other appurtenance of the *royal* manor of Amesbury so specified was a small estate at Lyndhurst, in the *royal* hunting grounds of New Forest. It is remarkable also that the two manors immediately preceding Lyndhurst in the Domesday folios for Hampshire should also include outliers in the Isle of Wight.⁴¹ Apart from Amesbury, there were altogether six mainland manors with outliers in the Isle

³⁶ See, for example, F. Morgan and P. Vinogradoff (eds.), *Survey of the Honour of Denbigh*, 1914.

³⁷ G. Bersu, 'Excavations at Woodbury, near Salisbury, Wiltshire (1938)', *Proc. Prehist. Soc.*, New Series, VI, 1938, 312. I am indebted to Mr. B. R. S. Megaw for drawing my attention to a subsequent paper by Dr Bersu ('Celtic Homesteads in the Isle of Man', *Journal of the Manx Museum*, v, Nos. 72-3, 1945-6, 177-182), in which he identifies structures of the Little Woodbury type as the dwellings of the aristocrats of Celtic society.

³⁸ J. W. Birch, *op. cit.* 47. See also G. B. Grundy, 'The Saxon Land Charters of Wiltshire', *Arch. J.*, LXXVI, 1919, 143-50. A further hint of continuity is provided by the Roman bricks used in the construction of Britford Church, a structure in which work possibly of the 8th century A.D. is still preserved. See *Victoria County History, Wiltshire*, II, 1955, 25-6.

³⁹ *Domesday Book*, I, f. 64b, 74.

⁴⁰ *Ibid.*, f. 52, 64b.

⁴¹ *Ibid.*, f. 38b.

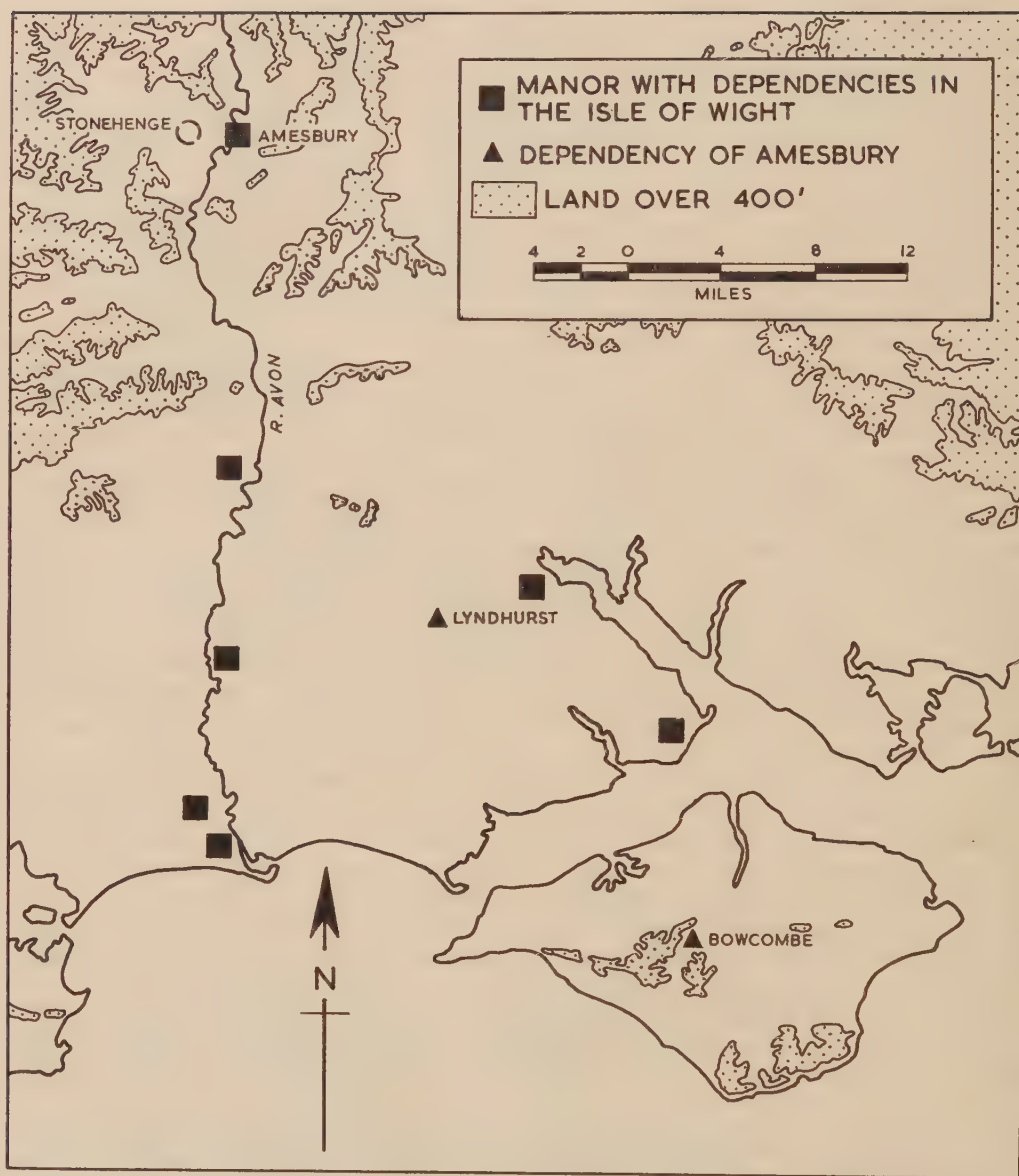


Fig. 4. *Mainland manors with Dependencies in the Isle of Wight.*

of Wight during the 11th century.⁴² Not unnaturally one of these manors adjoined Southampton Water, and another the Solent, but curiously enough the remaining four resembled Amesbury itself in that they were sited on or near the banks of the Avon. Like Amesbury, all six manors were protected by nearby hill-forts. Amesbury was the head of a Wiltshire

⁴² *Ibid.*, f. 38b, 39 44. I am indebted to Professor H. C. Darby for information concerning the number of such manors.

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hundred, but that it should have had such a distant outlier has always been regarded as inexplicable.⁴³ Yet is there not here a faint echo of that far flung control of territorial resources which the construction of the metropolitan sanctuary of Stonehenge must have entailed? ⁴⁴ May the curious disposition along the Avon of four other manors with dependencies in the Isle of Wight not owe something to the use of this river for transport at an early date? After all, the Hampshire Avon forms part of one route which, as Atkinson has suggested, could have been used for the transport of the bluestones of Stonehenge on their long journey from Pembroke.⁴⁵ If this is the case, then the organization of discrete estates was already well established in Southern Britain during the early and middle part of the second millennium B.C.⁴⁶

Acceptance of this extended view of the history of permanent settlement in the lowlands of Britain would mean, however, that the discrete estate and hamlet structure of settlement should not be regarded as features of only Celtic social organization. It would be unfortunate if in an article whose object is to undermine the claim that permanent settlement in the lowlands of Britain was essentially an Anglo-Saxon achievement, an equally mistaken belief in early settlement in Britain as an exclusively Celtic accomplishment should be substituted. The Celts were merely a ruling caste who imposed themselves, probably during the Bronze Age, on a pre-existing sedentary population and in turn of course were themselves supplanted or reduced to serfdom by later invaders. The retention here of the convenient term 'Celtic' should therefore not be taken to imply an exclusive ethnic affiliation for the discrete estate and hamlet structure of settlement.

Where the prehistoric period is concerned, these suggestions must, at present, remain speculative. For the first millennium of the Christian era, however, there is an accumulating body of evidence to suggest that the lowland hamlet, within the framework provided by the discrete estate, was the basic unit of social organization throughout Britain. That this was equally true of England as of Wales is indicated, for example, by the administrative and ecological contexts of the not inconsiderable numbers of settlements bearing key names like *wealh* meaning Welsh or *cyning* meaning royal authority over a *villa regalis*. Such evidence is not confined to the districts considered in this paper. Consequently the time is perhaps ripe for the formulation, if only as a working hypothesis, of a general theory on the evolution of settlement in Britain more in keeping with current thought than the Germanic thesis still overtly accepted.

As the widespread distribution of hill-forts in Britain implies, discrete estates comprising a number of hamlets were once to be found in every district of England and Wales. They were not entirely absent even on the heavier claylands of the West Midlands, though here they were few and far between as compared with eastern and western districts where lighter soils are more characteristic. The small nucleated hamlet, adjoined by a patch of open-field

⁴³ *Victoria County History, ibid.*, 60-1; *Victoria County History. Hampshire and the Isle of Wight*, I, 1900, 408.

⁴⁴ The Knowlton Circles, sanctuaries of the Bronze Age which have been well described as the Stonehenge of Cranborne Chase, similarly stand in what appears to be a significant relationship to Knowlton, *caput* of the hundred of the same name. This settlement has disappeared apart from the church, which stands within one of the circles, and one farm. See M. W. Beresford and J. K. S. St. Joseph, *Medieval England*, 1958, 50-2. Oddly enough, one of the barrows bore the Welsh-sounding name *venellys* in a survey of the 16th century.

⁴⁵ R. J. C. Atkinson, *Stonehenge*, 1956, 103-4.

⁴⁶ It is possible that some of the more favoured sites in our lowlands have been in agricultural use, with intermissions probably only for fallowing, for a time span of no less than fifty centuries. Clegyr Boia, near St. David's, Pembrokeshire, one of the few Neolithic homesteads recognized in Southern Britain, stands at an altitude of but 250 ft. in an area where nucleated hamlets of the traditional Celtic type are still the characteristic forms of settlement. A similar view is advanced in *A Matter of Time*, 1960, a survey summarizing a great deal of recent archaeological research on the river gravels of England, which appeared after this paper was written.

arable, provided a uniform basis of settlement, at least by the Early Iron Age. Subsequently the open fields of such hamlets were extended and with the passage of time came to be worked by progressively more elaborate field systems until enclosure supervened. Variations in the rate of change in these field systems gave rise to those regional variations described by Gray and later workers.⁴⁷ In England, therefore, clear traces of the infield-outfield system of the hamlet, and of discrete estate structure, are best found in 'forest' districts.⁴⁸ In south-eastern districts the agricultural economy of the discrete estate was intensified after the influx of the Belgae. Together with the elaboration of Romano-British villa economy this led perhaps to the cultivation of enlarged areas. Such early extension was perhaps responsible for the irregular field systems of Kent and East Anglia. West of the Jurassic scarp, however, where old hamlets had been few and far between, there was abundant scope for Mercian overlords to impose, at a later date, a more regular enlarged field system; here agriculture was probably intensified for commissariat purposes, during the period when Mercia faced a dual threat from Wessex on the one hand and Wales on the other. The so-called Midland System of Gray, far from being the normal form of field system in England, as is so often assumed, can be more realistically viewed as but one regional variant on the basis provided by an old-established hamlet structure.⁴⁹

A similar line of reasoning can equally well be applied to settlement form. The growth of hamlets into larger villages can be regarded as a concomitant of that extension of field systems already described. This was best achieved at the central settlement, the *caput* or *villa regalis* of the discrete estate, which usually occupied the best land in the vicinity. The re-arrangement of homesteads around regular village greens, which some students of settlement have regarded as an expression of Anglo-Saxon activity, can be regarded as part of this process whereby field systems were re-arranged and settlements enlarged. The oft-quoted example of Heighington in Durham,⁵⁰ with its regular green, can be explained most readily as an Anglian re-arrangement after the Celtic shire, of which Heighington was the *villa regalis*, had been taken over as a going concern by the Anglians. Often, however, since the discrete estate provided a sphere of influence for the *caput*, the central settlement became the site of a market; and subsequent urban development, as at Leeds, has obscured the original settlement structure. Sometimes more of the primeval structure remains and the curious double villages of the Vale of York, for example, are probably a but slightly modified heritage of the discrete estate.

The thesis advanced in this paper should make such variations, whether of field system or settlement form, much more readily intelligible than has been hitherto possible. Its adoption should facilitate the interpretation of settlement without that excessive multiplication of logical entities which the traditional theory on settlement entails. Where resources of easily cultivable land held in demand by would-be settlers are in limited supply, the multiplication of such entities is at best improbable.

⁴⁷ H. L. Gray, *English Field Systems*, 1915; M. W. Beresford, *op. cit.*, 21-49, 125-51.

⁴⁸ As, for example, in Morfe Forest, Shropshire, the Forest of the Peak, Derbyshire, and the Forest of Galtres, Yorkshire. Even here, however, these traces have been blurred by assarting and the creation of severalities.

⁴⁹ Other uniformities in field system in different parts of England may well have been imposed from above as part of the taxation policies of the component Dark Age Kingdoms of England. See S. Goransson, 'The regular open-field pattern in England as a counterpart to, or possible model for, Scandinavian 'solskifte', *Geografiska Annaler* (forthcoming).

⁵⁰ H. Thorpe, 'The Green Villages of County Durham', *Trans. and Papers* 1949, *Inst. Brit. Geogs.*, 1950, 158, 161, 176-7; A. E. Smailes, *North England*, 1960, pl. 22.

Notes and News

THE KNOSSOS TABLETS: AN ANSWER

Professor Palmer's discussion of the sources and the nature of the evidence for the dating of the Knossos tablets is a valuable introduction to the whole problem. It remains only to add that in handling this evidence a very high degree of precision is called for. Thus, when on p. 136 of the last number of *ANTIQUITY* Palmer assumes that the 'W. wall' mentioned in Mackenzie's notebook for 4 v 00 is in the Room of the Stirrup Jars (RSJ), it may not be without interest to observe that the sketch map for that day shows that the west wall of RSJ had not yet been traced (although it may have been meant). Also, that Palmer quotes from the inked fair-copy of the 1900 NB which Mackenzie made for Evans, and that in the original pencilled diary the words 'on the clay floor' do not appear at all. We shall see what led to this addition in the inked version in a moment, but this is, of course, not the main issue in this area, and what I have said simply illustrates the degree of accurate reporting which the evidence demands, but has not received as yet.¹

Let us look again, then, at the evidence of the excavation in the adjacent Room of the Saffron-Gatherer (RSG) as reported by Palmer. Briefly, he records for us that in 1901 two floors were found, with tablets on the lower one; and that in 1923 a plaster floor was diagnosed as LM III from its composition and the find of LM II pottery beneath the continuation of the same floor in the next room (to the south). On the evidence as presented it might seem reasonable to follow Palmer in identifying the 1923 floor (taken as LM III) with the lower floor (with tablets on it) uncovered in 1901. This is assumed by him without discussion; it is central to his argument; and it is wrong.

In 1901 the NB shows that it was the north half of the RSG which was dug and where the tablets were found on the lower floor. The lower floor itself was removed in this half of the room and the excavators dug deep into a walled pit of the Early Keep. This is made clear in the plans of 1901 (*BSA*, vii, pl. 1) showing the outline of the pits below the north half of the room. The southern half of the room was left at the level of its latest floor exposed in 1900, and it is this which was investigated in 1923. It could not have been the floor with the tablets on it since this was in the north half of the room, dug away into the Keep below. Moreover, the NBs show no record of tablets found above the late plaster floor in the southern half of the RSG or above the continuation of this floor to the south.²

If corroboration is needed let us look at the descriptions of these floors, which tell the same story, although the terminology is rather broad. Our latest floor is, in 1901, 'good white cement', in 1923, stucco or lime-plaster of a pale colour. The lower floor, with the tablets, is simply 'cement' in 1901; in 1923 Mackenzie describes this floor, below the 'stucco', as

¹ I should say that I have read carefully all the notebooks and other unpublished evidence in Oxford. Professor Palmer announces a forthcoming publication by him of the testimonia. I have also prepared a publication of the find-places of the tablets and evidence for re-occupation in the palace, using all available evidence, published and unpublished.

² By 1923 only parts of the floor here could be studied since the pits of the Early Keep in the rest of this area had been opened up in 1903 (*BSA*, ix, 22 ff.) and the later walls above supported on arches. On p. 138, n. 2, Palmer takes the late slanting wall to be the 'dividing wall' discussed by Evans in *POM*, iii, 17, but a careful reading of this passage shows that this is quite wrong, and his reflections on Evans therefore unjustified.

the typical 'terrazza' flooring of the LM II palace, such as was found in the RSJ next door ('a kind of concrete'). Again, a gypsum slab in the middle of the RSG lay between the areas dug in 1901 and 1923 and is mentioned in both years. In 1901 it forms part of the lower floor with the tablets, embedded in it but its top free above it. In 1923 it is observed as probably 'part of an earlier arrangement' and the late plaster or stucco is 'fitted on to it'. Finally, the eloquent attempt to equate the wrong floors does not absolve us from facing the recorded fact that in two places tablets were found beneath the latest Bronze Age floor,³ and so belonged to a period earlier than the final abandonment of the re-occupied parts of the palace (by c. 1200 from the pottery found in it).

And the RSJ itself? The tablets and 'the clay floor'? (Readers will have noticed that Palmer attributes the clay floor to the RSJ on p. 136, to the RSG on p. 139.) In 1900 the excavators did not observe that the clay above the cement floor in the RSJ represented a significant later floor level, but the report that the tough clay deposit was responsible for the poor preservation of the tablets (*BSA*, vi, 45; following Mackenzie) shows that the tablets were *in* or *under* the clay. The complete vases of late date must, of course, have stood *upon* it.⁴ This observation of the clay led to Mackenzie's addition in his inked diary (see above). The circumstances became clear next year when digging below and beyond the collapsed north wall of the RSJ showed tablets beneath the latest floor level.

Palmer's next crux is the North Entrance Passage (NEP) where whole LM III double-jars were found beside tablets. Adherents to Evans's dating are challenged to cite tablets with which whole LM II pots occur. The tablets, of unfired clay, are preserved by a conflagration which baked them. Some are blackened, almost vitrified. There are good scorched examples from the NEP in Oxford. What happened to the tablets must have happened to any pots stored with them *at the time of the fire*. This is why we have no whole LM II pots beside tablets! And why the intact and unburnt LM III pots⁵ beside the broken and burnt tablets do not afford a dating context. It is utterly naïve to imagine that objects found together were manufactured at the same time, especially when their conditions are so different. Evans thought that the tablets had been shifted in to the NEP (to make up the floor, I would suggest). The joining fragments of a tablet from the NEP and an adjoining room (cited by Palmer) support this view, as does the scatter of the deposit to the RSJ, and the haphazard contents of the 'archive' in the NEP. Add that the NEP was not an entrance at all in LM III, since it had been cut off by a wall which was removed in 1901, but appears in earlier plans. In LM III the entrance was a squatter's pot-store.

A more telling context may be Mr Hood's recent discovery of a clear destruction level of c. 1400 with burnt pottery immediately beside the Arsenal where Evans found a destruction level with burnt tablets. The complex is surely one and the same, and there was no later Bronze Age building there.

There are, of course, many other problems about the site which are relevant. And there are yet others which do not depend on the find-places at all. For example, the clay sealings, some countermarked in Linear B, which were found with the tablets and preserved by the same fire. The dates of the seals used can be established by the clear sequence of styles shown by finds in Crete outside Knossos and in mainland Greece, and this dates the latest

³ Passages cited by Palmer, p. 136. In case the word 'Mycenaean' misleads it should be remembered that the term 'Minoan' had not then been invented, and the whole structure at Knossos was called Mycenaean.

⁴ Palmer, p. 136, is right to say that neither Evans nor Mackenzie hinted at stratification in their sketch plans of the area. Add that in *none* of the NBs of these years is stratigraphy shown in plans, or sections drawn.

⁵ The vases are shown in photographs taken in 1900. Some have been identified in Crete by my colleague, Mr Popham, in April of this year. They are among a large number of such vases, all of which are intact and unburnt.

NOTES AND NEWS

of them to the opening phase of LM III⁶—the same period as the pottery in the palace debris.⁷ None is in the distinctive and degenerate style current at the end of LM III, some 200 years later.

Evans chose the RSJ area to illustrate the stratigraphy because it was the most obvious place where LM III re-occupation was stratified over the tablets. In itself this is significant. The overall picture is of (a) parts of the palace choked still with burnt debris and tablets, and (b) other parts which were cleared of the debris and re-occupied later in LM III, and where the late vases were finally abandoned unburnt. The reconstruction of an excavation at this remove of time is no easy matter and obscure points may easily be exploited by special pleading. Only a completely consistent story of the whole site, at variance with the basic conclusions drawn by the excavators, but using the evidence they carefully recorded, could establish the correctness of Palmer's theory. Professor Levi's strictures on the publication of Knossos (quoted by Palmer, p. 141) are only in part justified. But the volumes of *Palace of Minos* will need correction in many details. Fortunately the site was dug with more care and better records than any other Greek site in the early years of this century, and than many since. We must respect the available records by treating them with precision and understanding, and not cloud the issue by picking on discrepancies in Evans's later presentation of them. For our purposes *Palace of Minos* can be ignored, since we have the notebooks, and the reports in *BSA* which are a wholly fair account of the record in the notebooks.

At the end of his article Professor Palmer suggests that we must weigh Mackenzie's records against Evans's conclusions. And may we not even consider Mackenzie's own conclusions? Mr Hood has already quoted this passage of Mackenzie's (published, 1904) and it is not lightly ignored: 'In no deposit which was recognized as belonging to this period of partial habitation at Cnossus was a single inscribed tablet or seal, broken or unbroken, ever found during the whole course of the excavations there.'

JOHN BOARDMAN

⁶ Mr V. E. G. Kenna has recently studied again the sealings in Crete and wholly agrees with this dating. The argument is not a circular one since it depends on independent dating evidence from other sites. Unless, that is, it is argued that LM II vases and other objects were being made at the end of LM III *b*, a suggestion which does not warrant an answer.

⁷ It will be realized that it is convenience only that prompts the term 'LM II destruction', since the debris contains much of the earliest LM III style. Arguments from the record of 'LM III' pottery (as Palmer, p. 139) in the Stratigraphical Museum can only be admitted when the pottery is re-examined, since the period is a long one (perhaps 200 years) and the destruction at Knossos happened some time after its start. Mr Popham has made some important observations on such dating after studying the boxes.

EARTHWORMS AND ARCHAEOLOGY

In 1945 a re-issue of Darwin's book on earthworms appeared under the title *Humus and the Earthworm*; the work became immediately popular and caught the attention of many archaeologists, who saw the importance of the theories advanced there to their own work. In *ANTIQUITY* for December 1957, Professor R. J. C. Atkinson in his paper on *Worms and Weathering* made a comprehensive study of this subject, in the course of which he referred to the well-known fact that stones spread on the surface of a field will 'gradually disappear and after a lapse of several years will be found, still as an integral layer, at some distance below the surface' (p. 221). He continued with the definite statement that: 'The sinking of such material is wholly due to the activities of worms.' I should like to put forward the suggestion that the sinking of such material can and does take place in the almost complete absence of a worm population.

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After reading the re-issue of Darwin's book, and having reason from my own observations to doubt some of the conclusions put forward in it, I began a series of experiments, which have been continued over the past fifteen years. On damp, acid, mountain soil, which I knew contained remarkably few earthworms and those of a type that apparently never made surface castings, I threw down a number of small stones and watched them regularly to see what in fact happened to them. After three years they had vanished from sight, and they are now about 2 in. below the surface. The process by which they sank was perfectly clear and obvious, and it had nothing to do with the activities of earthworms.

This process, or chain of events, may be described in relation to a single stone for the sake of simplicity, and it will become evident that only small stones are affected—anything that may be described as a boulder, after sinking slightly under its own weight, remains firmly on the surface. The first thing that happens when the stone is placed on the grass is that it kills the plants underneath it, and so snuggles down into the sward; its presence, however, has a stimulative action on the plants growing immediately around it, caused, it seems likely, by the warmth it stores up during the day and radiates at night. These grasses, growing up more strongly as a result, extend their leaves into the space above the stone and their roots into the space, now unoccupied, below it: creeping plants also take advantage of the situation and creep out over the surface of the stone, beginning to cover it. Sooner or later a more potent and dramatic force inevitably comes into play, that force being generated by a grazing animal, usually a sheep or a cow, which drops on to the stone a smaller or greater quantity of manure; most of this is then washed off again by heavy rain, but some remains cemented on and is bound in place by the creeping plants, which extend fine roots into it. During the winter the coarser grasses growing round the stone die down, and the decaying fibres of the leaves remain there as added material to that already accumulating; when more manure from grazing animals falls it is retained in increasing quantities. Meanwhile the stone is sinking slightly in relation to the general turf level; this is partly under its own weight, materially aided at times by pressure from the feet of animals and occasionally man, but it must be assumed that material is actually removed from under the stone and brought up on top of it by the action of growth in the grasses and plants surrounding it, whose roots extract chemicals from the soil and pass them to the stem and leaves of which they form a constituent part.

It is not long before the sinking of the stone into the ground, combined with the collection of material on its surface, enables the actual grass sward to invade it and cover it completely; after that has occurred the humus steadily builds up by the continued process of the decay of vegetable matter on the surface reinforced by the grazing animals' droppings, which are from now on but slightly washed away by rain. At this point an occasional worm may play a useful part in the process, not so much by bringing up soil as by passing through the material already accumulated and improving its quality by its digestive functions.

It appears that the act of laying down a layer of loose stones over a grass sward will, when the conditions are suitable, bring about the raising of the general level of the area by an amount somewhat greater than the average thickness of the added layer; thus the depth of material measured from the new surface after many years have elapsed to the top of the stony layer is not all to be accounted for by the actual downward movement of the stones.

This leads to the consideration of a second aspect of the same phenomenon attributed mainly to the action of earthworms, namely the building up of layers of material overlying solid floors, particularly in the archaeological context, those of deserted buildings. Professor Atkinson says (*loc. cit.*, p. 222): 'In fact, such occupation earth is almost certainly the product of earthworms, which penetrate cracks in apparently solid floors and gradually

build up a layer of castings on their surface.' Useful sites for the study of this question are provided by abandoned hard tennis-courts, because their level surfaces are laid over a depth of at least one foot of graded clinkers, ranged from large at the bottom to small at the top, forming an environment impossible for worms to function in, and much too thick for them to force their castings through; thus the action of worms can be safely excluded, yet the courts become rapidly covered with a depth of humus when left untouched. Many courts were given up during the war, and I have been able to watch two of them during the past fifteen years with this problem in mind; one to which grazing animals have had access now resembles a normal field, unexpectedly resistant to drought; the other, entirely walled off, still has a few bare patches. The action in these cases begins under the influence of the wind, which blows dust, some rubbish, and a lot of dead leaves on to the surface, where some of each is held in corners, or by small irregularities in the surface, or is plastered down by the rain; this is sufficient to give a hold to small seeds, also blown in, which in due course germinate, and bind the material more firmly with their roots. In periods of drought this first vegetation dies off completely, but leaves behind each time that this occurs conditions more favourable than before for the repetition of the cycle. The addition of the droppings of grazing animals is here a potent factor in assisting the building up of a finally drought resisting layer of humus; since the site is level this will continue to grow in thickness, provided it is not subjected to other denuding forces.

In the case of deserted buildings the procedure may be slightly different, but the walls will help to trap material blown in through the windows and doors, and roofs do not last very long—those of thatch will, in their decay, add much to the layer of humus below. It may also be noted that domestic animals, if left to themselves, will regularly shelter in deserted buildings at night in the winter, or during heavy storms, and the depth of droppings that they so leave is considerable.

My conclusion is that, whatever may be the importance of the action of earthworms in some localities, the phenomena hitherto attributed solely, or mainly, to them can take place very nearly as rapidly, if not just as rapidly, in their absence.

C. A. GRESHAM

CIRCULAR BUILDINGS FOUND AT YANIK TEPE, IN NORTH-WEST IRAN

PLATE XXIX

A short season of excavations, supported by the University of Manchester and the British Academy, was carried out in the summer of 1960 at Yanik Tepe, one of a large number of prehistoric sites close to the shores of Lake Urmia (now called Lake Rezaieh) in Azerbaijan.

The excavations were necessarily restricted to a limited area and to only a few of the successive building levels in the mound, which is 16.50 m. high and about 300 × 200 m. in extent. The pottery belongs to the same wide cultural province as Eastern Anatolia and Trans-Caucasia,¹ and suggests a date of c. 2100–c. 1900 B.C. for these levels. The outstanding discovery was that of twenty-three circular structures, uncovered either in whole or in part (FIG. 1 and PLATE XXIX). With one exception these were all houses, and several of them included what were evidently more or less standard features, namely, bins, benches, niches and platforms, the last probably being intended for the preparation of food. A central post was found in some of these circular houses, especially in those belonging to the later of the seven main building levels excavated; thin internal partition walls also occurred inside these later houses. In size the round houses showed relatively little variation, ranging from 6.90 m. to 4.75 m. in external diameter; their walls are remarkably thin, usually about 0.30 m.

¹ *Anatolian Studies* VIII (1958), 164–75.

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thick, though some are more massive. There is strong and repeated evidence to indicate that the walls curved in towards the roof. Three of the houses have a small enclosure built up against one segment of the outside wall, used for storage rather than habitation, and probably not roofed in. The precise character of the roofing of these round houses is uncertain, but it must have been of very light construction, since there is a complete dearth of remains of heavy beams or of a great mass of fallen mud bricks. This rules out the

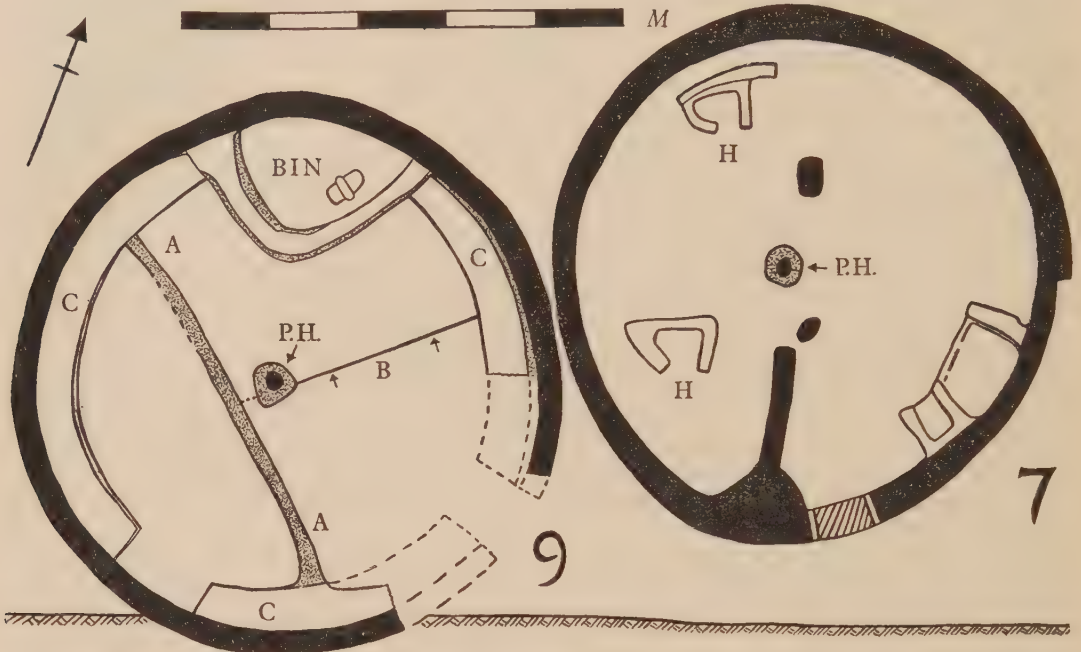


Fig. 1. Circles 7 and 9 (Level 2c). AA = Clay base of partition; B = 10 cm. step (along line of screen?); C = Plastered bench; H = Hearth; and P.H. = Post-hole.

possibility both of a flat roof supported on beams and of a domed roof of brick: the latter is anyhow made unlikely by the occurrence of central posts in some of the houses. A wickerwork frame, sometimes covered with matting, with a coating of mud, is the most probable method of roofing for these round houses: skins might have been used, but would hardly have sufficed to keep out the cold of the winters, when there is normally some snow. The narrowness of the entrances and the fact that the floor-level inside is usually slightly below that of the ground outside the houses likewise suggest that they were designed for protection as much from cold as from heat. The interior of these round houses must have been very gloomy, as in many village homes in the Near East today; some hint of this is provided by the discovery of four baked clay lamps, blackened with use.

The whole form of the round houses very much suggests an ultimate derivation from tents, which in turn implies that their inhabitants were of nomadic origin. A talent for wood-carving, betrayed by the shapes of some of the pottery and the incised decoration of much of it, was particularly marked, and is on the small scale of portable objects consistent with the life of nomads.

There is one circular structure of entirely different character from the rest, which are definitely houses. This has two concentric outer walls, the space enclosed by the inner one

being divided into four compartments, resulting in a rotary plan. There can be little doubt that the function of this building, which was preserved to a height of 2 m., was for storage; no actual grain was found inside, but at the bottom of one of the four deep compartments were several querns, which suggest, though they do not prove, that this was a granary. The most plausible explanation of the double wall, with a space in between, is that it was for insulation against damp. There was no entrance except from the top, so that it must have been filled and emptied thus, by means of ladders. It was evidently empty at the time of its abandonment. Its importance to the community is suggested by its prominent position, on the highest ground in that part of the site.

The numerous querns and pestles, apart from a polished stone mortar and some flint sickle-blades, indicate the importance of agriculture to this community; some chaff was found in the bin inside one of the round houses and in the enclosure belonging to another one. Animal bones occurred in some quantity, showing that stock-breeding was also of economic importance.

Although there were no objects that can for certain be described as imports, there seems to have been a repeated threat of attack, which extensive remains of burning show to have been at times successful. This has given the mound its modern Turkish name, 'the burnt hill'. The settlement, in this period at least, can be called a town rather than a village, since it was fortified by a massive wall, built of undressed stones set in mud, 5 m. thick. This wall had two main phases, and in the short stretch uncovered in the 1960 excavations a narrow gate-passage was discovered. Inside the wall was found a number of sling-stones.

The cemetery of this period, as yet unexcavated, lies on the lowermost slope of one side of the mound.

It is impossible here to say more of the pottery than that it is all hand-made, usually dark, burnished and with a great variety of shapes and decoration, always incised. Patterns are mainly in geometric bands or panels, but birds and horned animals are quite common, the only feature suggesting cultural connections with the rest of Iran.

The discovery of this settlement invites comparisons with other sites in the ancient Near East where circular structures have been found. The most obvious occurrences are the *tholoi* at Tell Arpachiyah² and Tepe Gawra³; there are also the beehive huts at Jericho.⁴ But the wide gap in date between these, especially the last, and the round houses of Yanik Tepe makes any close connection impossible. Moreover, the *tholoi* of Arpachiyah are stone-built and far more massive in construction than the houses of Yanik Tepe, although the *tholoi* of Tepe Gawra are more similar. None of the houses of Yanik Tepe had an entrance-passage or antechamber attached to it. The phenomenon of round houses has, however, been frequent in the Near East in various forms and at different periods, as, for example, with the beehive houses of North Syria today. It is to be expected that they will be found more widespread in parts of the Near East, with the progress of archaeological discovery, than they are at present known to be.

For close parallels with the round houses of Yanik Tepe one must look north to the Araxes valley and to the Caucasian region, where Russian excavations have revealed similar remains at Shengavit and Shresh-Blur (in the Armenian S.S.R.), at Beshtasheni (in Georgia) and at Kayakent (in Daghestan).⁵ At Shengavit there were houses each with a

² *Iraq* II (1934), Part I, especially Chapter 2.

³ A. J. Tobler, *Excavations at Tepe Gawra*, Vol. II (Philadelphia, 1950), especially 41-3, 47; pls. XLIIB and XLV.

⁴ E.g. *Palestine Exploration Quarterly*, 1952, 71.

⁵ For a brief discussion, see Alexander Mongait, *Archaeology in the U.S.S.R.* (Foreign Languages Publishing House, Moscow, 1959), 115-6.

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circular central room, with a diameter of about 7 m., against which rectangular rooms were built. These houses had a central post supporting a roof apparently of wickerwork. Thus, except for their stone foundations, these houses at Shengavit, etc., are closely comparable with those uncovered at Yanik Tepe. The settlement at Kayakent, like Yanik Tepe, was fortified: there the dwellings are described as 'built of stone and wickerwork plastered over with clay'. The economy was, moreover, similar to that of Yanik Tepe. Wickerwork walls were also found in the remains of huts at Dolinsk, near Nalchik,⁶ contemporary with the neighbouring Kuban burial mounds. Circular buildings, preserved in the form of models, were not unknown in the Tripolye culture of the Ukraine.⁷

Comparisons with sites in the Araxes valley and further north need not be confined to resemblances in architecture, since the pottery found at Yanik Tepe belongs, at least in many of its forms, to the traditions of the East Anatolian Early Bronze Age culture, which extended through Trans-Caucasia to Trialeti. For reasons to be given in detail elsewhere, a date of *c.* 2100–*c.* 1900 B.C. for the levels excavated at Yanik Tepe seems probable. The precise affinities of the incised decoration so prolific on the pottery of this site are as yet uncertain; a local origin is quite possible, though it is tempting to look again to Trialeti, where incised ornament was common at this period; but the patterns were not the same. The taste for incision on pottery, and even more the building of round houses, seem to indicate the likelihood that Yanik Tepe was occupied towards the end of the third millennium B.C., if not rather earlier, by people from the Caucasian region, who had at least been in contact with the inhabitants of the northern steppes.

A second season of excavations is planned for 1961.

CHARLES BURNEY

⁶ *Ibid.*, 116.

⁷ V. Gordon Childe, *The Dawn of European Civilization* (5th edition, London, 1950), fig. 71 (139).

THE NEWLY-FOUND GOLD TORC FROM MOULSFORD, BERKSHIRE

PLATES XXX, XXXI

This remarkable example of Bronze Age gold-work (PLATES XXX, XXXI) was found near Moulsoford, on the Berkshire side of the Thames between Oxford and Reading, early in 1960 by Mr F. J. Thomas of Aldworth, when working for Mr C. G. Venables of Moulsoford as a cultivator in one of his fields (*The Times*, 2 December, 1960). After an inquest in December by the Wallingford coroner, who declared the find not Treasure Trove, it has been acquired for the Museum and Art Gallery of Reading, whose Director, Mr T. L. Gwatkin, has kindly asked me to write this preliminary note on it for ANTIQUITY, while the site is being further explored.¹

The torc weighs about 1 lb. It is a composite affair, and yet quite simply made. Four 12-in. gold bars, about $\frac{1}{4}$ in. thick, shaped square and twisted spirally with an inch and a half at either end left plain, have been bent into the same penannular curve, laid two by two together, and held so by being thrust at each end into the grip of a pair of sheet-gold socket terminals. They are held together also by a binding of spirally-twisted stout gold wire, placed over to one side, so perhaps the survivor of a pair.

¹ Mr Gwatkin writes: 'The torc was offered on generous terms to the Reading Museum by Mr C. G. Venables, who wished it to remain in Berkshire. In making the purchase the museum was assisted by grants from the National Art Collections Fund, the funds administered by the Victoria and Albert Museum and the Berkshire County Council. The friendly advice and co-operation of the British Museum is also gratefully acknowledged.'

NOTES AND NEWS

Each of the twisted bars is really a single torc, of a kind well known from the Bronze Age, along with similar armlets and bracelets and even smaller ornaments, made in either gold or bronze. The ornamental twisting of wire or slender rod could be applied to gold bar quite readily, and simulated in bronze by 'mock-torsion' casting; in Central Europe, the mode is found from the mature Early Bronze Age onwards. In Bavaria the hoard from Regensburg-Hochweg, which contained a coiled gold bar so twisted, belongs to the Early/Middle Bronze transition, about the 15th century B.C.² Before the end of the Middle Bronze Age there, not only were mock-torsion bronze bracelets worn amongst its Tumulus people,³ spreading with their culture from South Germany into France as far as Burgundy,⁴ but both these and twisted gold ones were adopted in Northern Europe, and torcs in bronze similarly treated, whether still left plain at the ends or hooked.⁵ From there (perhaps with a side-wind from the Tumulus culture too), both bracelets and torcs were borrowed into Southern Britain, at the opening of the middle phase of the British Middle Bronze Age⁶; and when before long its influence reached Ireland,⁷ the types were rendered in the Irish gold. Of our twisted bar form with ends left plain, two armlets were found in the St John's or Castledermot gold hoard (Co. Carlow)⁸; another from Skelly, Drumnakilly (Co. Tyrone), threads a pair of little ribbed gold rings, for fastening its ends alongside one another.⁹ As for torcs of like form, there is a good part of one in the Coppeen gold hoard (Kinneigh, Co. Cork),¹⁰ while two exported to Southern Britain have been found in Kent, in the River Medway at Aylesford.¹¹

What we have now from Moultsford, a composite torc made of several such put together as a bunch of strands, is unique. It is not like the Nordic bronze *Halskragen*, where the strands have their length and radius graded, for the wearer's chest to show them off in series, each lying above the next. Its strands are bunched, and their ends are neither twisted together,¹² nor yet conjoined into a single pair of hooks. Terminals of hooked form are indeed known once proliferating into five strands, in the gold torc found in Brittany near

² Eckes, *Germania*, XXII (1938), 7-11; Powell, *Proc. Prehist. Soc.*, XIX (1953), 175; Torbrugge, *Die Bronzezeit in der Oberpfalz* (1959), 210 Taf. 74-5 (no. 347), and 40 *Ber. Röm.-Germ. Komm.* 1959 (1960), 53-4, 76 (no. 141). Gold in this bar form was of course meltable for other uses (Eckes, 11); yet Piggott, in *Proc. Prehist. Soc.*, IV (1938), 95, rightly here noted the start of its supervening on the earlier work confined to thin wire and sheet, though the Regensburg piece would be surprising if Irish work already so early.

³ Margaret Smith, *Proc. Prehist. Soc.*, XXV (1959), 149, and now e.g. Torbrugge, *op. cit.* 38-4, Abb. 11, 2 (his Phase C 1, which = Reinecke's late B, Holste's B2; chart, 14).

⁴ Nancy Sandars, *Bronze Age Cultures in France* (1957), 89-100, 103-4.

⁵ M. Smith, *ibid.* and 153; plain and hook-ended torcs, Kersten, *Zur ält. Nord. Bronzezeit* (1936), 36-8; Broholm, *Danmarks Bronzealder*, I (1943), plain in graves 1730, 1896, 2364, hooked elsewhere; both start within Montelius II but are most typical in III (c. 1250-1050 B.C.).

⁶ M. Smith, *op. cit.*, 144-64; plain-ended bronze torc, Hollingbury Hill hoard, Brighton, Sussex (B.M.), figured *Arch. Journ.*, v (1848), 324-5; date of this whole 'ornament-horizon' phase, or Middle Bronze 2, c. 1200-1000 B.C., or 1250-1050.

⁷ In bronze best represented by the Bishopsland hoard, Co. Kildare: ÓRiordáin, *Proc. Prehist. Soc.*, XII (1946), 161, pl. XIII; Hodges, *Ulster Journ. of Arch.*, XIX (1956), 32-3; dating, M. Smith, *op. cit.*, 150-1.

⁸ Armstrong, *Cat. Irish Gold Ornaments* (Dublin 1920), nos. 82-3 (pl. xiii, 105, 110), with plain penannular bracelet, 264 (pl. xviii, 376: Middle Bronze Age form, unexpanded ends) and two penannular grooved bands, on which see below with note 18.

⁹ *Ibid.*, no. 81 (pl. xiii, 99).

¹⁰ *Ibid.*, no. 80 (pl. xiii, 103), with small thick band and three penannular bracelets, one plain with unexpanded, one with slightly expanded ends and one with the same and body partly twisted: 456, 266, 253, 252 (pl. xiv, 240; pl. xviii, 386, 357, 395); theoretically, slightly expanded ends should be slightly later than unexpanded, but this is still essentially a Middle Bronze Age hoard.

¹¹ *Arch. Cant.*, ix (1874), 12, pl. A; Jessup, *Arch. of Kent* (1930), 113 and pl. x.

¹² As in the freak bronze *Halskragen* from Katerbow, Kr. Ruppín, N. Germany: Sprockhoff, *Jüngbronz. Hortfunde Periode V* (1956), 234-5, Abb. 43.

Quimper¹³; these however are not bars but ribbons, and all ribbon torcs are hooked. The hook-ended form of our bar torcs, as being more secure, was always more popular than the plain-ended form represented by the Mouldsford strands. The plain ends of this form itself, eventually, were converted to act as hooks by being bent back outwards, to produce the handsome outstanding terminals of the well-known 'Yeovil' or Tara type.¹⁴ To that type, in fact, belongs the only other multiple-strand torc known from the Bronze Age in the British Isles: that in the Towednack gold hoard from West Cornwall, where the strands are of tight-twisted slender rod, twined gently round each other and conjoined into a single pair of terminals.¹⁵ But no torc of the 'Yeovil' type has been dated earlier than what some have called Late Bronze 1, or in Ireland Late Bronze A,¹⁶ others in Britain a phase transitional to Late from Middle Bronze Age (Middle Bronze 3), but in either case belonging to the couple of centuries after 1000 B.C., and not before.¹⁷ And those torcs are so plentifully represented in Southern Britain that our type with unbent plain ends, which has never been found associated with them, should certainly be confined in date to the preceding phase, along with its counterparts in bronze and the simple hook-ended specimens in both metals. Its twisted binding-wire is likewise normal for that time. That is, the Mouldsford torc should be Middle Bronze 2, and belong to the century or two before 1000 B.C.; most probably to the 12th, when torc-making was still a new art in the British Isles.

The sheet-gold terminals, expanding slightly outwards (PLATE XXX), were formed of one sheet bent round on itself, secured at the outer end by the tapped-down edges of a second and circular sheet, which forms a cap (missing on what is in the photograph the left-hand terminal). The technique was being practised already in the Wessex culture of the Early Bronze Age,¹⁸ where, moreover, the base-cap of the conical gold button-cover from the Upton Lovel 'gold barrow' is traced with a quartered-circle pattern, directly anticipating that on the Mouldsford end-cap (PLATE XXXI(b)). The same grave's little gold 'boxes', capped in the same way at both ends, anticipate the larger Irish 'boxes', to which the 'bands' in our Castledermot hoard can have belonged¹⁹—though the only four preserved complete are probably Late Bronze Age.²⁰ The traced decoration on the body of the Mouldsford terminals (PLATE XXXI(a)) is of hatched bands and triangles offset by plain spaces, and has an Early Bronze Age ancestry most obvious among those of the Irish lunulas.²¹ Its freedom from concentric-circle or other Late Bronze Age elements is entirely consistent with our dating the torc well back within the Middle Bronze Age. So, too, is the absence of any solder on the terminals or elsewhere. The Thames valley population in Middle Bronze 2 was presumably related in culture to the 'Deverel-Rimbury' people best known in Wessex. The Mouldsford torc is an unexpected but not wholly surprising witness to their wealth and to their Irish contacts, in about the 12th century B.C.

CHRISTOPHER HAWKES

¹³ Mus. Arch. Nantes; Giot, *Brittany* (1961), 162. I must thank Monsieur Giot for a letter describing this, and certifying the absence of anything more like the Mouldsford torc from extant French literature or museum collections.

¹⁴ Savory, *Arch. Camb.* 1958, 3-12, 58; Maryon, *ibid.*, 52-6; superseding on technique my account in *Man*, 1932, 222 (which has full list to that date).

¹⁵ *Man*, 1932, 222, pl. H, 2.

¹⁶ Savory, *op. cit.*; Hodges in *Ulster Journ. of Arch.* 1954, 1956-7.

¹⁷ M. Smith, *op. cit.*, 181-5: palstaves of this 'transitional' phase accompanied the Grunty Fen gold armilla, of same type, *Archaeologia*, LXI (1908), 35.

¹⁸ Piggott, *Proc. Prehist. Soc.*, LV (1938), 79, with e.g. pl. vii, 6-6a (cone and base-cap), 8-9 (boxes), from Upton Lovel, no. 81; pl. ix, 9-10 (cone and base-cap) from Normanton, no. 71.

¹⁹ See note 7; the bands are Armstrong's nos. 377-8 (pl. x, 58-9), on which cf. his p. 61: the suggestion was Wilde's, as cited n. 2.

²⁰ Armstrong's nos. 371-4 (Frontispiece 485-8), with cabled mouldings and concentric-circle ornament both recalling the gorgets, which have no date by association any earlier.

²¹ Armstrong's pl. i-vii.

ARTISTRY AND ROMAN BRITAIN

In a recent review my old and valued friend, Sir Mortimer Wheeler, has discussed recent progress in the study of Roman Britain, somewhat to its detriment, with the suggestion that all the fun is now over, that minor adjustments are all that is needed or at least all that is being done. The amendment of one's predecessor's efforts occupy our time and form the principal justification of many works in this field. The major attainments of mankind must be sought elsewhere. Such is the burden of his theme, all highly understandable in the light of his own brilliant career, of which one of the most notable achievements was the education of India in the principle of selective excavation, itself one of the highlights of Romano-British studies.

Without depreciating the immense contribution which methods first taught in Romano-British archaeology, and still very well therein exemplified, have to make to British archaeological effort the world over, it remains possible, I believe, to take a less gloomy view. It is true that the major campaigns upon Hadrian's Wall, so far as they dealt with the sequence of its complicated series of works, have now come to a triumphant end. But there is still everything to learn about the sequence and varying dispositions and planning of its garrison throughout the centuries. No static organization, the Roman army: yet on what other frontier can the changes ranging over almost three centuries be so well or so conveniently studied? It was not Haverfield but his great colleague Mommsen who remarked that no province could teach the world more about the Roman army than Britain: and if that was true seventy years ago, it is no less significantly true today. As for the more northerly Scottish frontier, excavation here only begins to touch the kernel of its very history and its relationship with the barrier of the Lower Isthmus. These remain worthy themes indeed, especially when the opportunities and progress are compared with those which the other great barriers of the Roman world present. Nor does it seem right to refrain from mentioning that Northern Britain will in the now foreseen future present the world with a complete picture of a legionary fortress, together with the many otherwise unattainable calculations that are to be drawn from it, at Inchtuthil. It is indeed easy to say that such results are predictable in outline, that it is the broad picture which matters. But here St Jerome was right: *non contemnenda ea parva sine quibus magna constare non possunt*. The essence in such matters is to get the detail right.

No one in fact knows this better than my old friend. His own experience has taught him so. It is no accident that his splendid initiative at Verulamium now lies wide open to re-interpretation, not because others have reaped where he sowed, but because so much of the field was by him left fallow. This is no mere re-adjustment, it is a fundamental re-interpretation of the entire picture, and one which has set a-going the re-examination or new examination of many Romano-British towns. We get an entirely new view of the way in which these towns were allowed to develop with Imperial encouragement and with a positive native delight in fundamental alterations of the most drastic kind which rival any flights of fancy in planning for which the cities of the Greek East won an unenviable reputation. This is the kind of evolution in urban development of which no other city sites in Western Europe have produced evidence, for a variety of causes, one of the chief being lack of initiative and opportunity in excavation. It is no mean contribution to the history of the Empire that such facts convey, both as achieved knowledge and as a challenge or a contract. All this is the product of the last decade.

So too with the small towns and villas. These are, one may hardly doubt, included in the scornful quotation 'many shall run to and fro, and knowledge shall be increased'. But it is the last half of the quotation that matters, profoundly; and here he who laughs last, laughs

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best. The truth may well be, and I think we shall all agree upon this, that one of the besetting difficulties of Romano-British archaeology in the post-war years has been linked with the vast modern post-war developments in our island. Again and again, systematically planned work has had to give way before emergency work: and the difficult yet urgent problem of reconciling the two aims is one common to every branch of archaeology in the country. But emergency can sometimes be shaped into a plan. The most notable achievement in this regard is perhaps the post-war exploration of Chester and Lincoln, where work, started at Chester in emergency and Lincoln as a planned act, has since been shaped, now by planning and now by response to emergency, into a coherent picture of their development; not yet indeed complete, for desire must often wait upon opportunity, but so far advanced as to represent many chapters of a new book of life. Valuable indeed as such work is deemed by Sir Mortimer for training purposes, it is still more valuable for the vision which it gives of an instrument of intellectual inquiry accurately set and employed. It stirs the imagination the more vividly because it is part of our own heritage. It is water drawn from our own well for our own sustenance, and it was only after drinking of it fully that my good friend went out into the wider field in which he has so well employed the strength and power derived from it. Much is still to be drawn from the same source by those who will, and much of new and valuable import for British archaeology at home and abroad. *Sursum corda!*

I. A. RICHMOND

'THE ANCIENT FOREST OF BLACKAMORE'

In his recent paper on 'The Ancient Forest of Blackamore' published in *ANTIQUITY* (1961, 127), Dr Dimbleby draws far-reaching conclusions from five pollen-analyses distributed between two sites on the watershed of the Cleveland Hills, as to the influence of Mesolithic man in altering the vegetation of the area. At White Gill he found microlithic flints *in situ* 'lying flat upon the surface of the bleached mineral soil, underneath a layer of black organic matter about 5 in. thick, and . . . associated with a considerable amount of charcoal'. Above the black humus layer was a layer of brown peaty organic matter about 18 in. thick.

We are given pollen analyses 'from the surface of the mineral soil on which the flints were resting and the black humus horizon (0-5 in.) above the flints', and from consideration of them the author states that 'they indicate beyond all reasonable doubt that the Cleveland Hills watershed was forested in Mesolithic times', that the forest was becoming more open and that this was due to disforestation by fires initiated by Mesolithic man.

From a site at Burton Howes, also on the watershed but two miles to the west and 170 ft. higher, Dr Dimbleby gives three pollen analyses of turves associated with two Bronze Age barrows, and from them deduces that between the first building of Barrow 4D and its subsequent extension, the land around changed from being 'still largely forest' to being 'largely deforested'. This interpretation rests upon the asseveration (by no means generally accepted) that when the ratio of non-tree pollen to tree pollen is less than unity dense forest is indicated, and that values above 1.5 or 2.0 show open country.

No doubt Dr Dimbleby will expand the evidence for this in his forthcoming publication on the Development of British Heathlands and their Soils. A more serious criticism is that one cannot see upon what grounds Dr Dimbleby assumes that at White Gills the peat he analysed must be contemporary with the microlithic flakes that lie upon the mineral surface. It has long been recognized that in the Pennines the microlithic industries lie in an equivalent stratification, but archaeologists have acknowledged that they may have rested upon the mineral soil for an unknown, and possibly long period, before peat began to form over them. Not until Walker reported stratification of such microliths into the

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organic muds of a pool at Stump Cross was it possible to use pollen analysis and radiocarbon assay to date the industry (Walker, 1956).¹ Dr Dimbleby appears to rely upon the presence of the abundant charcoal to support the view that Mesolithic man set fire to the forests and indeed suggests that radiocarbon assay of the charcoal would date the culture, but had forest indeed returned to the White Gills area after the microliths were left there (as the author himself suggests is perfectly possible) there seems no reason why the charcoal and the lower peaty layer should not have formed at some later time and have the pollen spectrum of that later time. Strength is given to this suggestion by the very close resemblance between the tree pollen spectra of the lower sample at White Gills and of the samples from the Bronze Age barrows. Dr Dimbleby indeed points to their 'remarkable similarity'. He advances two possible explanations: that the Mesolithic and Bronze Age occupations may have been co-eval, or that after a long gap of time after the Mesolithic occupation, Climax forest was restored, only to be again destroyed by Bronze Age people. It seems to be overlooked that the second hypothesis, as we have pointed out, casts severe uncertainty upon the contemporaneity of the White Gills pollen spectra and the mesolithic artifacts at that site.

It might seem that all five pollen analyses cited here belong to the Bronze Age, and the crucial point would seem to be that only with the onset of peat accumulation over the mineral soil were there conditions in which pollen might be preserved. Is it not reasonable to suppose that it is to this episode that the pollen-analyses of the lowest organic layers will correspond, and not any earlier time?

H. GODWIN

¹ D. Walker, 'A Site at Stump Cross, near Grassington, Yorkshire and the Age of the Pennine Microlithic Industry', *Proc. Prehist. Soc.*, xxii, 1956, 23.

CONFERENCE ON THE IRON AGE IN NORTHERN BRITAIN

The Council for British Archaeology Iron Age and Roman Research Committee announce a Conference on Problems of the Iron Age (Pre-Roman and Roman) in Northern Britain to be held in Edinburgh from Friday, 6 October to Sunday, 8 October. The opening address will be given by Professor Stuart Piggott, and among the lecturers there are listed J. P. Gillam, George Jobey, J. R. C. Hamilton, R. B. K. Stevenson, R. W. de F. Feacham, E. M. Jope and K. A. Steer. The conference will end with a summary and closing address by Professor C. F. C. Hawkes. For further information write to the *Council for British Archaeology*, 10 Bolton Gardens, London, S.W.5.

TRAVEL STUDENTSHIP

The Secretary of the Royal Geographical Society draws our attention to the Lady Dorothy Mills Award of £1000 established by the Society as a result of a bequest from the late Lady Dorothy Mills.

The Award will take the form of a Studentship involving some element of adventurous travel or exploration, by land or air, tenable for one or more years within the limits of the sum available. Applicants (of any nationality) must be women over twenty-one and preferably under the age of thirty years. They must be Fellows of the Royal Geographical Society. The study which forms part of the candidate's programme may be in any field of Science or the Humanities, provided that it has some geographical content or aspect.

Applications must be made on the prescribed form and should be submitted by 12 February, 1962. They should be addressed to the *Director and Secretary, Royal Geographical Society*, 1 Kensington Gore, London, S.W.7, from whom forms of application may be obtained.

STONEHENGE: A SUGGESTION

The recent *Guide to Stonehenge*, by Prof. R. J. C. Atkinson, published by H.M. Stationery Office, raises some unanswered problems about the 'blue stones'. We may accept the evidence of petrologists that these stones came from the Presely Mountains, and the practical evidence, given in the *Guide*, that their transport was possible. The reader is left to assume that the intending builders of Stonehenge went to Presely, quarried the stones and brought them to the site on the Wiltshire Downs. No reason is assigned for this lapse into the larger lunacy. Nor, I think, could any reason be adduced. The only reason that I can imagine for this prodigious expenditure of labour is that the tribes of Wessex (to use an anachronism) made a large-scale attack on those of South Wales, utterly defeated them, and in order to mark the completeness of their victory and also to acquire additional spiritual strength, demolished and carried off, largely with the slave labour of the defeated Britons, an existing 'temple' of widely renowned sanctity. This, after all, is the basic tradition embroidered by Geoffrey of Monmouth. Certainly Geoffrey's standard of truthfulness was noticeably lower than that of George Washington: but it does not follow that there was no foundation for a tradition which seems to fit the facts, or at least the possibilities.

The question is—where are we to look for this non-existent monument? Fortunately, holes in the ground are among the most indestructible of archaeological remains. There are some sixty blue stones at Stonehenge; allowing for a fairly heavy percentage of losses in transit, there ought to be a site marked by perhaps 80 to 100 holes. These one would expect to take the form of a single or double circle; though the possibility of avenues such as are found in Brittany cannot be ruled out. I suggest that a meticulous examination of air photographs covering say twenty miles or so round Presely might reveal the site. As a mere guess, I should favour the Newport-Fishguard region—a district strong in megalithic remains—and especially such sites as Dinas Head and Pen Caer; these seem suitable for a ritual sanctuary, they would be convenient for sea transport, and their position would fall in with the early tradition that the stones of Stonehenge came 'from Ireland'.

L. F. SALZMAN

Professor R. J. C. Atkinson writes: There is nothing new in the idea that the stones were brought to Stonehenge as the *disiecta membra* of a pre-existing monument; but it would be necessary to accept this hypothesis only if it could be shown that on arrival at least some of them had already been dressed to shape. Other possible explanations are discussed briefly in my *Stonehenge* (original edition of 1956, 173-5; Pelican edition, 175-6).

ART IN ROMAN BRITAIN

PLATE XXXII

This Exhibition (to which reference was made in ANTIQUITY 1961, 85) was held in the Goldsmiths Hall in London from 26 June to 22 July, and was admirably displayed and well attended. A full illustrated account of this exhibition, edited and with an introduction by Professor Toynbee, is being published by the Phaidon Press next year. We publish here (PLATE XXXII), in a fine photograph kindly provided by the Gloucester City Museum, one object from the exhibition—the stone head of a man dating from the first century A.D. found at Gloucester itself. It is 8 in. high and still bears traces of red colouring; the catalogue to the Exhibition refers to the Celtic traits namely 'the patterned hair, bulging lentoid eyes, stylised ears, slit-like mouth and tapering face'. The head was first published in *Trans. Bristol and Gloucester Arch. Soc.*, 1934 (78 and pl. VIII), and again described in the *Journal of Roman Studies*, 1935 (218 and pl. 37), and by Sir Thomas Kendrick in his *Anglo-Saxon Art* (1938) where (p. 13) he calls it 'a surprising piece'. Perhaps the interest and surprises in this Exhibition may stir somebody to organise an exhibition of pre-Roman art in Britain. 10,984 people visited Goldsmiths Hall in the 23 days of the Exhibition.

Reviews

HIGHLAND FOLK WAYS. By I. F. GRANT. London: Routledge and Kegan Paul, 1961.
pp. 377, 80 figures. 40s. net.

Dr Grant is well known for her writings on Scottish social history and for her pioneer work in establishing the Highland Folk Museum at Kingussie. Familiar both with the documentary sources and with the material evidence of traditional folk culture, she is unusually well equipped for her present task, within the limits she has set herself. Her attitude to the subject follows naturally from a life-long interest in human relationships throughout the centuries since the later Middle Ages, an interest strengthened by family memories and by her own rich experience gathered in the Highlands and Islands. The charm of this book lies in the personal approach: it does not claim to be scientific and indeed, judged by the exacting standards of Scandinavian folk life research, it falls short. The illustrations, too (by Molly MacEwan) admirably clear and varied as they are, show neither locations nor scales and are not always happily married to the text. The museums where the objects may be seen are not given, nor, in most cases, the authors from whose sketches copies have been made. The author makes no claim to scientific training, and is on shaky foundations in her discussions of the physical environment. More serious are the acceptance of outmoded views on unilateral cultural evolution and the reluctance to see Highland folk culture as anything more than 'a degradation of the old Gaelic civilization' (p. 27). The backcloth of her stage is the clan system. There is nothing here of prehistory or of the wider anthropological concepts which made Arthur Mitchell's book, *The Past in the Present*, a remarkable achievement and gives it value after more than eighty years.

This said, *Highland Folk Ways* has much to commend it. Dr Grant is refreshingly objective on such controversial topics as the kilt, tartans and Highland games, which by and large owe their popularity to the influence of Sir Walter Scott, the romantic movement, royal personages, Highland regiments and sentimental Lowlanders with one eye on the till. With insight and anecdote she can penetrate to the core of the Highland character. What could be a more delightful measure of time than the number of songs required 'to finish waulking the web'? One recalls Dr Delargy's Irishman's reference to a remembered incident which happened 'when the dew was thinking of falling'. A reviewer can best show the interest of the book by selecting a few items which held his attention. The curved scythe sned, usually regarded as one of the many technological improvements by which America repaid its debt to Europe, is stated to belong to 'the old days' in the Highlands (p. 109). The trump (the Jew's harp) held a special place in Highland lore because, being made of metal, it could safely be used in providing music to appease the fairies. One wishes that the art of feathering the staves in stave-built vessels, said to be necessary if they are not to collapse when dry (p. 212) had been fully described; I confess I have never seen it on vessels I have handled. The 'sledge' illustrated on p. 282 and described on p. 281 is really a slide car or, in Mitchell's words, 'a cart without wheels'. Dr Grant is at her best in describing traditional cloth-making and clothes and a number of interesting ecological relationships emerge. The new-fangled kilt, for example, was unsuited for many country tasks, and particularly for fishing, and had almost died out by 1840 (p. 326). The bunched skirts of crofter women were admirably adapted to provide support for the back-creel (p.

330). Curiously, no reference is made to the reputed Dutch origin of the wind-resistant tam-o'-shanter. Finally, one would like to think it is true (but can it be?) that 'all over Scotland, girls still wash their faces in the dew at dawn on the first of May' (p. 357).

Dr Grant's book is certain to have a wide appeal and it would be a fitting reward for a life's devoted work if Scotland were to follow the example of Wales and Northern Ireland in establishing its National Folk Museum.

E. E. EVANS

AN INVENTORY OF THE ANCIENT MONUMENTS IN CAERNARVONSHIRE.

By THE ROYAL COMMISSION ON ANCIENT AND HISTORICAL MONUMENTS IN WALES AND MONMOUTHSHIRE. Vol. I: EAST. *London: H.M.S.O., 1956. pp. lxxviii + 215, 188 text-figures, 100 plates. 65s.* Vol. II: CENTRAL. *London: H.M.S.O., 1960. pp. lxvi + 287, 194 text-figures, 82 plates. 90s.*

The publications of the Royal Commission in Wales have a long but not entirely happy history. The early volumes were deplorable even by the standards of their time, and were hurried into an unlamented demise by a memorable and probably pseudonymous review of the Pembrokeshire Inventory in the first volume of *ANTIQUITY*, which fittingly contained a denunciation of another archaeological fraud, at Glozel. Thereafter the Commission was reconstructed for the production of the Inventory of Anglesey, which appeared in 1937 and conformed for the first time to the standards already adopted in England and Scotland.

The first of the two volumes here reviewed was published after a further lapse of nineteen years, and both it and its successor presumably set the pattern of presentation for some time to come. Until the appearance of the third volume on Caernarvonshire, containing the sectional prefaces, it is difficult to assess the full measure of the Commission's achievement since the war. But meanwhile it is not too early to enquire how far the present volumes improve upon their predecessor, and to what extent they fulfil the needs of those who will use them now and in the future.

The maps at the end, overprinted with the numbers of the monuments, have been omitted, and probably rightly, since every entry has its appropriate grid-reference. The arrangement on a territorial basis by parishes, however, has been retained, though the Commissioners are clearly aware of its disadvantages. For the future its abandonment in favour of a classification by types of monument, already adopted in Scotland for Selkirkshire, will be heartily welcomed by everyone except the historians of individual parishes. A useful move in this direction has indeed been made in these two volumes, by increasing the number of categories within the parish framework from three to eighteen.

Another praiseworthy innovation is the adoption of a uniform series of scales for plans, which greatly facilitates comparison, even if occasionally at the cost of obscurity of detail (e.g. no. 380). It is also good to see the inclusion of so many minor examples of domestic architecture. But it is perhaps a pity, in view of the current interest in the typology of roofs, that these are illustrated too often only by plans, which are themselves too frequently cluttered up with stencilled information about the chamfering of beams and the like.

This device has been adopted as a matter of policy, to economize on the text; and it has been used as well for the lesser archaeological sites, some of which are accorded no more description in the text than a bare title, such as 'stone circle'. In such cases one might legitimately hope at least for an identification of the rock used (information which is given as a matter of course for later buildings); and also, where appropriate, an estimate of how deeply these early stone constructions may be buried in peat. This is usually easily found with a probe, without the necessity for any digging.

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In volumes such as these, which must clearly serve as the definitive account for at least a couple of generations during which the questions asked by scholars will change and multiply, it is essential to err in description on the side of liberality. A more generous attitude would have permitted the inclusion of a measured drawing of the only cup-marked stone in the area, which is mentioned but not illustrated; and, more particularly, of the Early Christian inscribed stones, the photographs of which were obviously taken for the most part under very difficult conditions of lighting.

The introductory section of the first volume contains a useful discussion of the Graig Lwyd group of neolithic axe-factories, and in both stray finds are listed and admirably illustrated by drawings. The principal categories of site are shown in a series of distribution-maps which in content compare most favourably with their predecessors in the Anglesey Inventory, though in style and execution they are unfortunately inferior. Their legibility is impaired by failure to isolate the numbers and symbols even marginally from the stippled background of the base-map.

The plates are ample in number, though perhaps less than generous to the archaeological sites, especially in Vol. I. Their quality is mainly good, and often excellent. But there can be little excuse for the lack of sharpness in some of the photographs, and none at all for the deliberate use of an unnecessarily distracting background for some of the examples of church plate (vol. II, pl. 35).

The one feature of the presentation of this material which actively invites censure is the lack of any uniformity in the conventions and lettering of the plans. The latter comprises a most irritating mixture of styles and sizes, discordantly juxtaposed, often clumsily executed, and in many cases chosen without apparent regard for the scale and balance of the drawing as a whole. Admittedly there is a comparable variety of lettering in the Anglesey volume; but its much greater elegance makes its inconsistencies unobtrusive. It is surely desirable, if only for ease of comparison, that comparable monuments, such as the castles of Conway and Caernarvon, or Cricieth and Dolbadarn, should be delineated by uniform conventions and lettering. To ask for this is not to imply, needless to say, that any one set of rigid conventions can or should be used for the whole range of monuments.

This is something that should be corrected, if only because a greater sensitivity towards the visual presentation of the evidence would be more worthy of the diligent and devoted labours in the field which have brought these volumes into being. There are standards of literacy in draughtsmanship as well as in writing; and where both methods of communication are essential, a common standard is essential likewise.

R. J. C. ATKINSON

THE ENGLISH FARMHOUSE AND COTTAGE. By M. W. BARLEY. London: Routledge and Kegan Paul, 1961. pp. xxi, 297, 24 plates, 38 figures, glossary and tables. 55s.

This book is divided into five parts, dealing with housing in England from the Middle Ages to 1690 with a discussion in the final part on the death of the vernacular tradition. It has an appendix of select documents, including probate inventories and extracts from parsonage terriers.

Books on English houses in the past have been the work of architects, antiquaries and archaeologists. Mr Barley is an historian and his book depends almost completely upon written records such as parsonage terriers, probate inventories, local histories and various published papers and books. It is the result of much patient and thorough research; the superstructure which Mr Barley has built upon this research is indeed excellent. But this

is not to say that Mr Barley is merely an armchair historian: his text shows that he has examined many houses. Through research and examination, he has produced a magnificent work which fills a large gap in the sparse literature of this subject. Is it too much to hope that this book will become an essential textbook in all the departments of history in universities and colleges? In the past the professional university historians (with a few brilliant exceptions) have been far too prone to dismiss works on material culture as outside their field.

Mr Barley, in his treatment of medieval houses, refers to the defended enclosure containing the hall and also separate buildings for other purposes; he refers from time to time in the course of the book to the absence of a kitchen from several hall houses. Here the Welsh Laws are illuminating. They state that the villeins had to build for their king 'nine houses' specified as a hall, a bower (*ystauell*), a kitchen, a chapel, a barn, a kiln, a stable, a court-house and a privy. A kitchen outside the house, and a continuance of the tradition, should not be regarded therefore as extraordinary. Indeed, in certain Welsh areas (see *Man*, XLIII (1943)) the practice of cooking in a pot-oven outside the house persisted until recently. Mr Barley gives no reference to the pot-oven in England; this is a subject worthy of further investigation.

The discussion of the 'third room' (i.e. in addition to the hall and chamber) is excellent; the backhouse is a feature of Welsh as of English houses. But I would suggest that it is dangerous to imply that it may once have been a cowhouse or that the connection between them is plain (p. 76). On p. 110, the author rightly suggests that the great width of the cross-passage in Devon and Somerset houses 'is due to its origin as a way through which cattle were brought into the byre'. This disposes of the strange argument (A. D. Rees, *Life in a Welsh Countryside*) that the space might have been used for threshing! A Welsh example of the wide cross-passage can be seen in Cilewent, a house now rebuilt in the Welsh Folk Museum.

My admiration for Mr Barley's work is so great that I shall not be misunderstood when I query some of the prefatory statements in this volume. 'The study of small houses', states Mr Barley, 'is . . . a problem for the archaeologist' and he proceeds to show that 'the historian can step in' to help him. By 'archaeologist' is meant the prehistorian and those who depend upon excavation for evidence. I cannot agree that their discipline equips them in any way for the study of houses. Archaeologists like Sir Cyril Fox (to whom this book is dedicated) are quite exceptional. It is the student of folk life (or material culture, ethnography, human geography, or social anthropology, call it what you will) who is best qualified to carry out this work. In the same context, Mr Barley writes of the importance of distribution maps: two of his own (figs 11 and 37) are truly admirable because they are based on a real wealth of data. But distribution maps can be not only useless but positively misleading. Innocent's map showing the distribution of crucks is an example, and can anyone argue that industrial east Monmouthshire was so completely devoid of significant houses as the maps in *Monmouthshire Houses* suggest?

Mr Barley states, quite rightly, that 'England still lags behind the other countries of North-Western Europe in the study of folk culture'. But as far as Great Britain and Ireland are concerned, he cites in contrast merely the Irish Folklore Commission (which has been primarily concerned with the folktale). I am sure he must be aware of the excellent work on material culture in Scotland based on the School of Scottish Studies and on the National Museum in Edinburgh, and in Wales, of the work of the Welsh Folk Museum (which sponsored, as the title-pages show, the three volumes of *Monmouthshire Houses*). Nor in fairness to English workers should the comprehensive survey of farmhouses and cottages in the north-west of England, undertaken by the staff and students of the University of

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Manchester's School of Architecture, be overlooked; it is an achievement which many other Schools would do well to copy.

One further point of considerable substance needs to be discussed. In this work, Mr Barley has adopted the division of Britain into Highland and Lowland Zones and has based the whole framework of his excellent study upon this arbitrary binary classification. This is to be regretted. It was the late Sir Halford Mackinder early in this century who propounded the theory of highland and lowland zones in Great Britain. He had in mind a broad physical-geographical classification of which there can be no criticism (see, for instance, 'The Uplands and Lowlands', chap. v, in his *Britain and the British Seas*, London, 1902). He wrote as a geographer about physical geography. But when Sir Cyril Fox revived the theory in 1932 (*The Personality of Britain*), he used it not as a self-evident physical-geographical principle, but as a decisive factor in the movements of prehistoric invaders and their cultures, and indeed as a basis for contrasts between cultural fusion and replacement. The application of the broad generalizations of physical geography to particular human problems is always dangerous. The original adaptation, however brilliant as a theory, becomes debased in the hands of subsequent writers who try to fit their own particular problems into its mould. The Highland-Lowland Zone theory has now resulted in a conception of Britain which may fairly be called a false dichotomy. In the process, a false emphasis is unavoidable. For instance, Mr Barley maintains that 'the lowland zone had advantages . . . of close and easy connection with the Continent'—this, one is led to assume, in contrast with the 'highland zone'. But we know that in medieval times the greater part of the 'highland zone'—Wales and Scotland—was in very close contact with the Continent. The closeness of these connections is explained by the fact that at that period Wales and Scotland were independent nations; the reasons were political and human, not those of geography. When Wales was united administratively with England, it was natural that many of her contacts with the Continent should then be *via* London. This was not because topography was the prime factor in these cultural exchanges; it was the result of human behaviour and historical events. In the same way, it is preposterous to suggest that 'the culture of the highland zone is found to consist of an amalgam of features of varying age' all originating 'in other and more prosperous regions', always, I suppose, lowland! Such facile pontifications are unworthy of such a brilliant study as Mr Barley has given us in this work, which I commend to all students of our cultural past.

IORWERTH C. PEATE

ATTIC GEOMETRIC WORKSHOPS (Yale Classical Studies, vol. xvi). By JEAN M. DAVISON. *New Haven: Yale University Press*, 1961. pp. xi + 161, 64 plates, 3 figures. \$5.00.

The detection of the painters who decorated Greek pottery is a favourite pursuit of Classical archaeologists, and during the last twenty years some of them have been trying their skill on those products of the later Geometric school of Athens—roughly of the second half of the 8th century B.C.—on which the more impersonal abstract ornaments give place to animal and human figures. Davison's investigation is wider than those of her predecessors; she has studied more than 800 of these figured pots and attributed about 140 to painters and workshops. For the leading painters much of the work had already been done; here she makes some good additions and adjustments. Her new minor painters are more difficult, but I should guess that about two-thirds of her attributions will stand. On the relationships between the various painters Davison is generally sound, in detail not very clear. Nearly all the pots and sherds she discusses are illustrated photographically in her plates, though some—because of scale or angle—do not show relevant details.

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This is a useful study, but it might have been more useful. There is no index of the pots which are discussed, and cross-referencing is incomplete. The system of classification—with dual entries—is too elaborate. The arrangement of the material—first early, next late, and then intermediate groups—tends to confuse the reader, the more so because the author does not use dates to indicate the temporal relations between them. The text, though, is clear and special terms are defined in the introduction.

Davison confines herself almost completely to style, and quite rightly. Her comments on interpretation of subjects are few and mostly sound, but imply the reader's prior knowledge of Geometric 'perspective'. In her excursion into chronology the arguments about the beginnings of Geometric are very weak. The book is well printed and well bound, and the price is very reasonable.

R. M. COOK

PROBLEMS OF THE IRON AGE IN SOUTHERN BRITAIN. Edited by S. S. FRERE.

Papers given at a Council for British Archaeology Conference held at the Institute of Archaeology, 12–14 December, 1958. *University of London Institute of Archaeology Occasional Paper, No. 11*, 1961. pp. xii and 308, 14 plates and 28 text figures. 30s.

It is well known that there is no modern work of reference on the pre-Roman Iron Age in Britain, and one must go back to the classic *Kendrick and Hawkes* of 1932, and to R. A. Smith's *British Museum Iron Age Guide* (1925) to find anything that might be regarded as a textbook. The present collection is not of course comprehensive, but it marks a very important stage in published studies on this period; one which it must be hoped will lead to a companion volume on the northern part of the island, and to new books at the hands of our acknowledged specialists. Both for its content and format, the Editor and the sponsoring organizations are to be congratulated.

First comes Professor Hawkes's 'The ABC of the British Iron Age', already published in *ANTIQUITY* (1959, 170–182). The 'ABC' system has stood the test of thirty years' usage, and has now proved itself capable of considerable elaboration. Some may be daunted by such docketts as 'Eastern Second B' which overlaps Periods II and III, but it must be faced that archaeology, like all other forms of investigation, becomes increasingly complex, that new terminology has got to be mastered, and that Professor Hawkes has not saddled us with anything that cannot be revised or discarded in fifty years time when the present intermediate phase in Iron Age research should be over. The 'ABC' system may not be the best for all periods of British prehistory, but it is an admirable working method for the six centuries of the Iron Age. The geographical provinces and regions now introduced into the system have been defined with great understanding, and the reviewer, writing within the Pennine Province, terrain rarely quartered by wandering scholars, would seek no frontier revisions.

In a symposium on smaller settlements, everyone will be glad to see a plan of the temple excavated by Professor Grimes at Heathrow, but they will be distressed that no illustrations support the notes on Staple Howe, East Yorks., where Mr Brewster found Hallstatt C razors (of just what kind?), nor on Castell Odo, Caernarvonshire, where Mr Alcock excavated a quantity of pottery. Nor for these three sites have any indications been given as to the appearance of definitive reports. Next follow three papers to do with hill-forts. Mr A. L. F. Rivet discusses the difficulties of classifying hill-forts for cartographical purposes, and goes on to clear our minds about the ways in which the defences could have been constructed. His case for the insular development of close-set multiple ramparts is convincing, but a superficial reading of this paper must not lead to a disbelief in sling warfare. The

slingers' platforms and stones survive, but they came later. Lady Fox contributes a clearly documented paper on south-western hill-forts, especially those with wide-spaced ramparts, and develops an interesting interpretation of their economic and social causes. She points to certain analogous hill-forts in North-Western Spain, but remains cautious as to inter-connection pending further archaeological links. Mrs Aylwin Cotton describes types of hill-fort in South-Western Britain other than Lady Fox's, and other distinct types in the south-east, mainly in Kent and Essex. This refinement of classification on structural and geographical grounds is a process which must eventually be applied over the whole country, and should lead to important indications of spheres of building technique, if not of tribal or confederate groups. This, with pottery classifications, should go some way to providing political and cultural maps for the centuries earlier than the evidence of numismatics and Classical geographers.

Writing on the beginnings of La Tène ornamental style in the British Isles, Mr E. M. Jope brings into sharper focus the conditioning factors, stylistic and technical, for the birth of the insular craft. The case for warriors arriving already accoutred with La Tène style weapons and ornaments grows weaker, and it is now shown that Iron Age 'A' chieftains had employed craftsmen working in La Tène I modes. Mr Jope's comments on the significance of Ireland's earlier pieces are particularly interesting, together with the view that western seaways had been of prime importance in conveying La Tène art to these islands. Mr Frere contributes one of the most stimulating papers in the collection. He is concerned with a complete reappraisal of the course of events, and of internal chronology, from the end of the 2nd century B.C. to Caesar's expeditions. His arguments, based largely on numismatic considerations, are most acceptable, and this paper is skillfully cross-referenced to that of Mr Allen's noticed below. A leading conclusion is that the Belgae were already settling in South-Eastern Britain by the turn of the 2nd and 1st centuries B.C., that chariotry was still in vogue in Gaul when they departed so that they brought this practice with them and continued its use up to their encounters with Caesar. Mr Frere suggests that chariotry was given up in Gaul in face of the superior freedom of movement of Cimbric 'cavalry'. It is probable that 'mounted warriors' would be a more appropriate term. Archaeologists generally should be more careful in the use of the term 'cavalry' which implies regular, drilled, formations. It is well, however, that the limitations of chariotry should at last come to be recognized. They were never the cross-country 'panzers' so often claimed. For events on the south coast, Maiden Castle B, and Glastonbury, can now be confidently assigned to a date earlier than 56 B.C., and Durotrigan B to some point in the mid-2nd century B.C. All along the chronological difficulties that appeared unduly to retard British Iron Age cultures, as compared with neighbouring continental sequences, are fading away.

Mr Brailsford follows with a short note of observations on the study of Iron Age pottery, and particularly stresses the need for better standards in published reports.

Finally, there is Mr D. F. Allen's supremely important 'The Origins of Coinage in Britain: A Reappraisal'. This contribution, with its copious appendices, occupies more than half the whole volume. Mr Allen can now demonstrate that all British coinage, with certain few exceptions, derives from Belgic exemplars, but the introduction of a large body of coins actually struck in Gaul began in the late second century B.C. The distributions of these coins most usefully show the source of the immigrants, their first colonies, and, in the British derivatives, their ultimate territories. The distribution of these various coin sequences and families is not coincident with the accepted geography of tribal names and powers of the second half of the 1st century B.C., so that good reasons must in future be adduced if tribal names are used in connection with the early phases of the Belgic settlement. Eight plates of coins support the text, and the apparatus of lists and classifications,

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make this paper indispensable. Quite apart from internal British matters, there are to be found many comments and facts of value on Celtic coinage in general.

T. G. E. POWELL

ISCA: THE ROMAN LEGIONARY FORTRESS AT CAERLEON, MON. *By* GEORGE C. BOON. *Cardiff: National Museum of Wales, 1960. pp. 1-38, with 12 plates and a plan. 7s. 6d.*

The third edition of the National Museum of Wales guide to Caerleon appears in a new format and under an altered title. It has been well re-designed with a gay cover and an eye for an attractive page; the plates have been relegated to the end, and Mr Sorrell's water-colours now appear in colour as a frontispiece. The text, which includes discoveries up to 1959, has been completely re-written by Mr Boon. It describes the opening stages of the conquest of Britain and the foundation of Isca; the history of the fortress and the visible remnants of it; and its place in the Roman occupation of Wales. This general and easily read account is followed by a condensed list of excavated sites and structures, with full references to excavation reports and to the site-plan at the end of the guide. Care has been taken throughout to aid the reader, and more especially the visitor to the site. He is not given fussy directions, but there are useful references to such mundane matters as bus-stops and stiles; his attention is lucidly drawn to significant objects in the Legionary Museum (also re-arranged by Mr Boon); and he is wooed by the clarity and style of the whole production.

D. R. WILSON

DIE MEGALITHGRÄBER DES IBERISCHEN HALBINSEL. *By* GEORG and VERA LEISNER. Part I: Der Süden. Römisch-Germanische Forschungen, Bol 17, *Berlin, 1943. Textband, pp. vii + 618, 29 figures; Tafelband, 71 double plates, 89 single plates, 6 tables and 2 maps.* Part II: Der Westen. Madrider Forschungen, Vol. I, 1st part, *Berlin, 1956. pp. viii + 122, 69 plates and 11 maps.* 2nd part, *Berlin, 1959. pp. xix + 349, 56 double plates, 28 single plates and 17 maps. No prices given.*

The appearance of the fourth volume of this massive Corpus of Iberian Megalithic monuments marks the near conclusion of a self-imposed task of epic magnitude. It is now about thirty years since Georg and Vera Leisner began the studies which resulted in this work, of which one may say at once that it has put all present and future prehistorians of Europe under a permanent obligation to them. They have given us the solid foundation on which all future studies of the problems of Iberian megaliths must be built, and added one more to the already long list of distinguished contributions by German scholars to Hispanic studies. Unfortunately, only Frau Leisner has survived to receive our congratulations on this occasion, which puts the seal on a remarkable working partnership of husband and wife during nearly half a lifetime, and of which this work is the durable monument.

When the Leisners began their work it had already been long evident that a proper understanding of the development of the collective tomb cultures of Spain and Portugal was of vital importance not only for Peninsular prehistory, but also as the key to the interpretation of much related material from many of the other countries of Western Europe. Yet although a considerable body of material, the result of many decades of excavation, already existed for study, appallingly little of this evidence was actually available to scholars in published form, and what did exist was extremely scattered. Thus theorizing and controversy were carried on largely on the basis of the plans of a relatively few large and famous tombs, which were reproduced again and again, and a sprinkling of grave-goods, whose relation to actual tombs was often uncertain, plus whatever other information about tombs

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and their contents each individual student had been able to collect by personal observation or correspondence.

Lacking a proper groundwork of fact to build on, scholars either arranged the available material in neat but subjective patterns, or despaired of doing anything with it. Real progress was largely impossible until the material and the factual information available should have been collected, sifted, arranged and published. This need the Leisners set themselves to meet, and it is no exaggeration to say that their work has completely transformed the situation.

They conceived their task to be more than this, however, and in the course of their work they formed opinions about many aspects of the material they were dealing with, which they have published, notably in the first volume of the *Corpus* and in other books and papers. Their views have always met with the respect which they deserved as being based on a profound and detailed first-hand knowledge of the material, and many have been seminal in the development of the theoretical reconstruction of this period of Iberian pre-history. If sometimes their conclusions seemed to be based too exclusively on minute considerations of typology to be completely convincing, yet the scrupulous and accurate evaluation of the evidence on which they rested could never be lightly dismissed, while some of their most important ideas have now become the commonplaces of Iberian archaeology.

After all, though, it is not in the theoretical conclusions to which their work has led that its greatest and most permanent importance lies. The real core of it is the painstaking record, in drawings, photographs and words, of the form and contents of hundreds of tombs and groups of tombs all over the Peninsula (with the sole exceptions of the Catalan and Pyrenean tombs, which had been already systematically studied by Pericot before they began work). To this essentially pedestrian, but exhausting, task they brought infinite patience, indefatigable energy, and an approach at once systematic and intelligent. An important instance of this last quality is their practice of combining drawings of tomb-plan and grave goods on the same or adjacent pages, greatly simplifying the use of the work by giving the user the maximum information with the least trouble. We find it displayed once more in the selection of the most significant combinations of objects to be plotted on the splendid series of distribution maps. But in fact examples of both this and the other qualities mentioned will be noted by the constant user in every detail of their work.

Though basically the same layout is used, the two parts of their work, published at such a long interval, inevitably show some different qualities, due partly at any rate, to differences in the documentation on which they were based. The first part, which, though it did not become generally available until after the war, was the product of work done in the years immediately preceding it, resulted from extensive field and museum work, not supported or amplified by original excavations by the authors. True, they were able to make large use of the records made by Louis Siret and his foreman Pedro Flores (who alone was present for most of the time) of a long series of excavations carried out over many years, that gave much valuable detail which might otherwise never have been published. Still, the imperfections of these records have naturally left their mark on this part of the work. For instance, the recent Spanish excavations at Los Millares have been able to correct some of their conclusions about the structure of the tombs there which were based on the Siret records rather than direct observation. Such details, however, detract little from the value of the work as a whole.

The second part, dealing with the tombs of the Western part of the Peninsula, is the fruit of a still more prolonged study which this time included campaigns of excavation in Eastern Portugal and in the Huelva district of Spain, the results of which were first

published *in extenso* in separate reports, and the authors were also able to profit by the renewed activity of Spanish and Portuguese archaeologists in the late 'forties and 'fifties. The first volume of this part contains a general study of the characteristics of the Western tombs and the problems which they raise; the second, recently published, is entirely devoted to an inventory, by regions, of tombs and their contents. Strictly speaking, to complete the pattern of the first part, there should be a concluding section on the grave goods, chronology and general conclusions. But while this would be of great interest, the corpus itself, which is the most vital part of the work, is now complete.

Much is being done, and much more will undoubtedly be done in the coming years to advance still further our knowledge of the Iberian Megalithic cultures. But whatever discoveries are made, and whatever changes come in our theories about them, this great synthesis, carried out with such devotion in the face of many discouragements and difficulties, including the six stormy years of World War II, will remain the basic tool of all students of these problems for many years to come, and a permanent monument to the courage and perseverance of its authors. We look forward to the publication of the fifth and final volume of this great work—which is in the press at the moment.

Finally, however, I must record a hope that we shall not always be mainly dependent on the evidence from graves for our knowledge of the collective tomb people in Iberia, and that the present promise of a great period of settlement-archaeology in the Peninsula will be fulfilled, yielding us a more balanced picture of their personality and their activities.

J. D. EVANS

Book Chronicle—*continued from p. 180.*

- PIONEERS OF PREHISTORY IN ENGLAND. By L. K. CLARKE. pp. 112. London, Sheed and Ward, 1961. 5s. (No. 10 in the series the *Newman History and Philosophy of Science*.) A brief and very interesting account of British views with regard to the antiquity of Man between Frere and 1859. At last does justice to Father MacEnery and is stimulating and very fair on Buckland. A must for all students of the history of archaeology.
- DIE VOLLGRIFFSCHWERTER DER URNENFELDERZEIT AUS BAYERN. By H. MÜLLER-KARPE. pp. 134 with 103 figures, maps and plates. (No. 6 of the *Münchner Beiträge zur Vor- and Frühgeschichte*.) Munich, C. H. Beck'sche Verlagsbuchhandlung, 1961. DM. 60.
- THE BIBLE AND THE ANCIENT NEAR EAST: Essays in honor of William Foxwell Albright. Edited by G. ERNEST WRIGHT. pp. 409. London, Routledge and Kegan Paul, 1961. 50s.
- BEGINNING IN ARCHAEOLOGY. By KATHLEEN M. KENYON. pp. 228, 9 plates, 14 figures. London, Phoenix House, 1961. 16s. The second revised edition of this book (first published in 1952) with sections on Archaeology in America by Saul S. Weinberg and Gladys D. Weinberg.
- KULTURA NASELENIYA TSENTRAL'NOGO ALTAYA V SKIFSKOE VREMYA. By S. I. RUDENKO. pp. 163 text figures, 128 plates of which 13 in colour. Moscow/Leningrad, Akademiya NAUK CCCP, 1961. English price 25s. Rudenko's second major study of the Scythians.

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